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ABSTRACT

This report focuses on young mothers who had children as teenagers, who had dropped out of high school, and who were receiving Aid to Families with Dependent Children. It was a voluntary demonstration project that provided comprehensive education, training, and other services intended to increase the long-term self-sufficiency and well-being of these mothers and their children. The evaluation of New Chance is one of the few large-scale, rigorous evaluations of programs designed to change the outcomes for this population. This is the last in a series of reports from the study. A variety of community-based organizations implemented the program well in 16 diverse sites, although participation by the enrollees was uneven. At the 18-month follow-up point, the program had created a substantial increase in educational attainment, with acquisition of a General Educational Development certificate by many participants, greater use of good quality child care, and improvement in parenting skills, balanced against high rates of repeat pregnancy, inconsistent program attendance, and the fact that more than 80% of the participants were still on welfare. A monograph based on 50 interviews with participants explored some of the circumstances behind these findings. This report extends the study to 42 months of follow-up. The 2,079 young mothers who were studied in the follow-up are now 22.4 years of age on average. For many measures, outcomes have improved for these young women since they enrolled in New Chance, but the sobering news is that the absolute levels of progress leave these families far from self-sufficiency. For most outcomes, New Chance did not improve progress over and above that shown by an equivalent group of young women who did not attend New Chance. Although the New Chance experience provides few definitive answers about what should be done, it does raise critical questions about the direction and consequences of public policy, and it does indicate the need for public policies that move beyond the scope of the welfare system to enhance young mothers' efforts to become self-sufficient. (Contains eight tables.) (SLD)

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New Chance

Final Report on a Comprehensive Program for Young Mothers in Poverty and Their Children

Janet C. Quint

Johannes M. Bos

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Preface

The New Chance Demonstration, and this report, focused on a population of great concern as the nation implements the new welfare law: young women who have children as teenagers and are high school dropouts. New Chance was a voluntary demonstration project that provided comprehensive education, training, and other services intended to increase the long-term self-sufficiency and well-being of these mothers and their children. While this approach is very different from that expressed in various large-scale welfare reform strategies, its lessons on the complexity of young lives lived in poverty challenge all who are interested in change.

The demonstration was developed in the mid-1980s, when the problem of unwed teenage childbearing was growing, but solutions were lacking. Research showed that young mothers who were high school dropouts constituted the group at highest risk of long-term welfare receipt, and like many older welfare recipients they were unlikely to be able to earn more than they received on welfare unless they acquired more skills. A number of funders and program operators embraced the New Chance approach as one that promised to address both the needs of young families and society's concern with the increasing rates and costs of out-of-wedlock births. The findings presented in this report speak to that concern and also to the related issues of targeting scarce welfare reform dollars, preparing young school dropouts for work, and assisting highly disadvantaged children of teen mothers.

The study of New Chance is one of the few large-scale, rigorous evaluations of programs designed to change the outcomes for this population. This is the last in a series of reports from that study. Earlier reports described a mixed picture of effects. A variety of community-based organizations implemented the program well in 16 diverse sites, yet participation by the program's enrollees was uneven. Some came and stayed, but others attended sporadically or dropped out after a brief period.

At the 18-month follow-up point, the program had created a substantial increase in educational attainment (acquisition of a GED, which is frequently a prerequisite for occupational training programs), greater use of good-quality child care, and a modest improvement in participants' parenting skills, balanced against high rates of repeat pregnancy, inconsistent program attendance, and the fact that more than 80 percent of the young mothers were still on welfare.

A monograph based on in-depth interviews with 50 former New Chance enrollees pointed to some of the circumstances behind this behavior: jobs found and lost, unplanned pregnancies, ambivalence about the balance between work and parenting responsibilities, and the important role played by family members, partners, and peers in supporting or undermining the young women's efforts to move forward. It offered moving testimony that behind the statistics is a group of young women who are determined to build a better life for their children but who, with few resources and little support, are frequently stymied in their progress.

This report extends the story to 42 months of follow-up. The young mothers are now 22 years old, on average, and their children range from infancy to schoolage. The report addresses the question of whether the 18-month effects have translated into gains in employment, reductions in time on welfare, and improved outcomes for the children of program participants.

On many of these measures, the outcomes for the mothers have improved since they enrolled in New Chance. The sobering news is that the absolute levels of progress leave these young families far from self-sufficiency, and for most outcomes the New Chance program did not improve progress over and above that shown by an equivalent group of young women who did not attend New Chance. Furthermore, the New Chance experience has created some unplanned, small, but troubling effects for some of the most fragile families in the study. For example, mothers at high risk of clinical depression at the outset were, at follow-up, at greater risk of depression and reported higher levels of parental stress than their counterparts in the control group. They also believed that their children were doing less well on measures of social behavior.

Taken together with the results from studies of other approaches to improving the lives of young, poor mothers who are dropouts, these results speak to the importance of developing earlier interventions that succeed in preventing the constellation of poverty, academic failure, and nonmarital teen births. They also speak to the continued progress, striving, and tenacity of the young families, and their need for support as they struggle.

The New Chance Demonstration has been a remarkable partnership of many funders, states, and local programs. We are greatly indebted for their support and the cooperation of the young women in the New Chance study.

Judith M. Gueron
President

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The authors of this report are deeply indebted to the young women in the research sample, who shared the information about their experiences and perceptions upon which the report is based. Also indispensable were the commitment and cooperation of the staff at the 16 New Chance sites. They regularly provided both quantitative and qualitative information on program operations, and their familiarity with both the program's workings and the lives of participants helped us understand the meaning of the statistical findings. For their review of an earlier draft of this report and for their assistance in updating the site profiles that appear at the report's conclusion, we owe special thanks to: Jo Rita Gann, Anthony Hermosillo, Bridgette Hicks, Robin Ingram, Catherine Liesman, Meri Maben, Stephen MacIsaac, Trissa Raiman, Constance Riedy, Kayci Rush, Jean Sabharwal, Robert Shaw, Anna Street, Linda Taylor, Mala Thakur, Stephanie Vendig, Rose Wallace, and Victoria Willingham Wilson.

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George Cave designed and carried out several of the nonexperimental analyses presented in the report and Chris Bost also contributed to the quantitative analysis. Mary Farrell, assisted by Kara Balemian, accomplished the massive task of collecting and analyzing detailed cost data from the program sites. Marilyn Price, Sharon Rowser, and James Healy provided ongoing information about site activities.

Vicki Hong was responsible for most of the data management and analysis with Christine Schwartz responsible for supplementary data analysis. Rachel Cytron, assisted by Simone Andrews, Eduardo Antonetti, Rachel Hitch, and Karen Trister, conscientiously and ably attended to the innumerable details of report and table production.

Nina Gunzenhauser edited the report. Patt Pontevolpe and Stephanie Cowell were responsible for word processing.

The Authors

Executive Summary

New Chance, a national research and demonstration program that operated between 1989 and 1992, was developed in a policy context marked by intense concern about teenage childbearing. That concern reflected the public's distress about three developments: the dramatic increase in the rate of out-of-wedlock childbearing over the past three decades, the long-term welfare costs incurred by young, poor women who become mothers, and the negative life prospects faced by their children. Little was known, however, about what kinds of programs and policies could help young mothers on welfare attain economic independence and could foster their children's development as well.

The recent enactment of a federal welfare law, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, is likely to alter the welfare policy environment in several important respects. For one thing, it has the potential to sever the connection between early childbearing and high expenditures for public assistance by imposing time limits on the use of federal funds to support cash grants to most needy families, including those headed by mothers age 19 (or age 18, if they are not enrolled in school) and older. For another, it provides financial incentives to states that reduce their rates of out-of-wedlock pregnancy.

Against this changed backdrop, the New Chance Demonstration provides findings—about the behavior of young mothers who are receiving welfare, the problems they face, and their efforts to move toward self-sufficiency—that are highly relevant to the new welfare scenario. The evidence suggests that states will continue to confront substantial challenges in helping young mothers find jobs and move off welfare before the time limits on their receipt of aid have been reached.

The New Chance Demonstration was a rare and important opportunity to test the value of comprehensive services in assisting a disadvantaged group of families headed by young mothers who had first given birth as teenagers, who had dropped out of high school, and who were receiving Aid to Families with Dependent Children (AFDC).[1] The program, which operated in 16 locations (or "sites") in 10 states across the country, sought to help the young mothers acquire educational and vocational credentials and skills so that they could secure jobs offering opportunities for advancement and could thereby reduce, and eventually eliminate, their use of welfare. It also sought to motivate and assist participants in postponing additional childbearing and to help them become better parents. Finally, New Chance was explicitly "two-generational" in its approach, seeking to enhance the cognitive abilities, health, and socioemotional well-being of enrollees' children. The program was, for the most part, voluntary; that is, young women were generally not required to attend in order to receive public assistance. Instead, most joined it because they wanted to earn their

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[1] Prior to the 1996 federal welfare legislation, Aid to Families with Dependent Children was the nation's largest cash welfare program. The legislation radically altered the structure of federal welfare by replacing open-ended federal matching funds for public assistance grants with block grants to states that the states can use for cash grants to needy households and for other purposes. The legislation sets a five-year time limit on the amount of time for which most adults can receive federal assistance dollars; states can exempt up to 20 percent of the assistance caseload from this time limit but can also impose shorter time limits if they so choose.

General Educational Development (GED, or high school equivalency) certificates and the program offered free child care to enable them to participate.

The program model was spelled out in guidelines developed in consultation with academicians, program operators, and other experts. The experts' recommendations reflected the prevailing view that earlier programs with a limited focus (for example, perinatal health care or education) had been largely inadequate and that a comprehensive intervention was needed to respond to the complex problems that young mothers commonly face. Accordingly, the model called for participants to receive a wide array of services addressing the young women's multiple roles and needs as students, prospective employees, mothers, family members, and partners. The services included instruction in basic academic skills and in subjects covered on the GED test, career exposure and employability development classes, occupational skills training, work experience, job placement assistance, health and family planning classes and services, parenting workshops, and "life skills" classes on communication and decision-making skills. These components were intended to reinforce and complement one another; together they were to convey a consistent set of messages about education, work, childrearing, and personal empowerment.[2]

The program model and demonstration were developed by the Manpower Demonstration Research Corporation (MDRC), a private nonprofit, nonpartisan organization that develops and studies initiatives to improve the well-being and self-sufficiency of poor people. MDRC designed and carried out the research agenda, provided initial training and ongoing technical assistance to the demonstration sites, helped them secure modest amounts of supplemental funding, and monitored their compliance with the program model and the research.

To evaluate the program's effectiveness, young women who applied and were determined to be eligible for New Chance were randomly assigned to one of two groups: the *experimental* group, whose members could enroll in the program, or the *control* group, whose members could not join New Chance but could receive other services available in their communities. To ascertain both short- and longer-term program effects, comparable information was collected from each member of both groups through in-home survey interviews conducted approximately 1½ and 3½ years after the individual had been randomly assigned. The measured average differences between the two groups' outcomes over time (such as their differences in rates of GED attainment, employment, or subsequent childbearing) and between the outcomes for their children are the observed results (or *impacts*) of New Chance. This, the final report on the New Chance program and its impacts, examines the trajectories of 2,079 young mothers who responded to the 3½-year survey.[3]

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[2] Like most interventions that focus on individual participants, New Chance sought to help enrollees understand and cope with the larger world, not to change the larger social, economic, and political environments in which the young women and their children lived.

[3] In all, 2,322 young women were randomly assigned, 1,553 to the experimental group and 769 to the control group. Although not all of them could be located or were willing to take part in the follow-up surveys, the response rates were very high for such research: 91.4 percent of the members of the experimental group and 89.2 percent of the members of the control group took part in the 3½-year interviews.

In addition to the follow-up surveys of research sample members, the report draws on data from several sources: the New Chance Management Information System (MIS), which contains the most detailed source of information on experimental group members' activities while they were in the program, a mail questionnaire completed by the preschool and regular school teachers of sample members' children, interviews with program coordinators and other

(continued)

The Impact Findings in Brief

At the time of the 3½-year interview, the young women were, on average 22.4 years old, and most had children who were still toddlers. Contrary to the common stereotype of these young mothers as immobilized by—or content with—their circumstances, the evaluation found that over the 3½-year follow-up period the young women in the research sample—experimental and control group members alike—were moving forward in many ways. At baseline (that is, random assignment), fewer than 10 percent of sample members had a high school diploma or a GED; by the 3½-year point, almost half the sample had earned one of these credentials. Sixty-three percent of sample members did not work at all during the year prior to random assignment; in contrast, over half *were* employed at some point during the 12 months before the 3½-year interviews, and the large majority of those who worked did so for 30 hours a week or more. These rates of employment are surprisingly high given the young age of the mothers and the fact that most had very young children. Over the follow-up period, the proportion of sample members receiving AFDC dropped considerably (although the majority were still on the rolls at the 3½-year interview), the proportion of women who used a reliable method of birth control rose steadily, and fewer women were at risk of depression. Nevertheless, the large majority remained poor and on welfare after 3½ years.

Although experimental group members received more varied services in greater quantity than did their control group counterparts and received them sooner, the differential was not large, especially with regard to education- and employment-related services. This is partly because during the period of the demonstration many education and training programs were available in the New Chance communities, and members of the control group participated in these in unexpectedly high numbers. At the same time, because of absenteeism and early departures from the program, members of the experimental group received on average a much lower intensity and duration of services than had been anticipated, and many never participated in skills training, work experience, or job search—the activities in the program model most closely related to employment.

The New Chance evaluation is not, therefore, a test of extensive services compared with no services or minimal ones. Rather, the evaluation measures the effectiveness of a particular mix and level of services that were relatively easy for those in the experimental group to obtain against another mix and level of services that individuals in the control group could secure only if they displayed somewhat greater initiative.

The findings indicate that while experimental and control group members both advanced in many ways, experimental group members did *not* advance further than control group members in most respects. New Chance did boost participants' levels of GED receipt above those of the control group. The added services provided by the program, however, did not help participants secure skills training credentials, get and maintain employment, or reduce their rates of welfare receipt or subsequent childbearing relative to outcomes for control group members. The program did not improve their children's preschool readiness scores, and it had unexpected small but negative effects on participants' emotional well-being and their ratings of their children's behavior.

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key personnel concerning program operations, site visit reports completed by MDRC staff members, and data collected for the analysis of the program's costs.

These results are puzzling, for MDRC observers judged all the sites to offer some high-quality services, and the large majority of young women in the experimental group said that they liked the program and benefited from it. It is likely that many factors, sometimes working in combination, account for the absence of impacts and for unanticipated impacts; different explanations may hold for different outcome areas. The possible factors include the slender differential in service receipt between experimental and control group members, the low absolute amount of services received by those in the experimental group, the possibility that some direct program effects produced unanticipated side-effects, and constraints on the magnitude of impacts imposed by larger social and environmental forces. It may also be that the program model itself was inappropriate for many young women.

These findings, unfortunately, are consistent with the results of other evaluations of programs serving young mothers on welfare who do not have a high school diploma or a GED, and the unsuccessful records of these programs highlight the importance of continuing to seek effective ways to assist these young women in improving their lives. But the impact results do *not* mean that the services New Chance provided (and that control group members received on their own) were of no value. Additional analyses were conducted to estimate the effects of service receipt for experimental and control group members together. While less definitive than the analyses undergirding the impact estimates, the results suggest that young women who received more than 18 weeks of education were far more likely to earn GEDs than those who did not and that young women who received skills training and attended college earned higher wages than their counterparts who did not receive postsecondary education or training. These findings held true even after other differences between those who received more or fewer weeks of education, and those who attended training or college and those who did not, were controlled statistically.

Thus, the findings indicate that the combination and quantity of services that New Chance participants received, on average, did not result in improved outcomes vis-à-vis those achieved by control group members. But they also suggest that receiving adequate amounts of specific kinds of services can make a difference in the mothers' lives—a finding of considerable importance to program operators and policy makers.

The remainder of this Executive Summary describes the young women who came forward for New Chance, their progress, and the issues they faced. It outlines the New Chance model as conceived and as put in place. It reports the impacts for the research sample as a whole and for particular subgroups of sample members. Finally, it comments on the lessons and cautions that the findings suggest for program operators and policy makers.

The Program's Service Structure

The 16 local New Chance sponsors shown in Table ES-1 were, in the main, community service organizations and schools and school districts. They also included a community college, a family service center overseen by the county government, a collaboration between the school district and the Job Corps, and a Private Industry Council (the local entity that distributes federal job training funds allocated under the Job Training Partnership Act). Although most sites selected for the demonstration had some previous experience serving young mothers, none was operating a

Table ES-1

The New Chance Program Operators

Location	Program Operator	Type of Organization	Prior Emphasis
California (Chula Vista)	Del Rey Center, Sweetwater Union High School District[a]	Adult school	Adult education
California (Inglewood)	Southern California Youth and Family Center[a]	Community service organization	Counseling, health services
California (San Jose)	Independence Adult Center, East Side Union High School District	Adult school	Adult education
Colorado (Denver)	Technical Education Center–North Campus, Community College of Denver	Community college	Adult education, occupational skills training
Florida (Jacksonville)	The Bridge of Northeast Florida[b]	Community service organization	Family planning, health services, tutoring
Illinois (Chicago Heights)	Aunt Martha's Youth Service Center, Inc.[c]	Community service organization	Counseling, adult education, employment preparation, health services
Kentucky (Lexington)	The Family Care Center[d]	Agency overseen by county government	Prevention and treatment of child abuse and neglect
Michigan (Detroit)	Development Centers, Inc., Community Mental Health Center	Community service organization	Mental health services
Minnesota (Minneapolis)	RESOURCE, Inc.[e]	Community service organization	Occupational skills training, employment preparation
New York (Bronx)	National Puerto Rican Forum, Inc.[a]	Community service organization	Adult education, English as a Second Language, occupational skills training
New York (Harlem)	Mid-Manhattan Adult Learning Center, Office of Adult and Continuing Education, New York City Board of Education[a]	Adult school	Adult basic education, GED preparation, occupational skills training
Oregon (Portland)	PIVOT–New Chance Program, Portland Public Schools	School/Job Corps	K-12 and adult education
Oregon (Salem)	Teen Parent Program, The YWCA of Salem	Community service organization	Recreation, adult education, child care, counseling, health services
Pennsylvania (Allentown)	Expectant and Parenting Youth Program, Private Industry Council of Lehigh Valley	Private Industry Council	Adult education, life skills, personal development
Pennsylvania (Philadelphia)	Lutheran Social Mission Society/Lutheran Settlement House Women's Program	Community service organization	Adult education, life skills
Pennsylvania (Pittsburgh)	Young Mothers Program,[f] The Hill House Association	Community service organization	Comprehensive services for teenage parents

NOTES: [a] A New Chance program is no longer in operation at this site.

[b] This agency was formerly named Family Health Services.

[c] This agency is now located in Park Forest, Illinois.

[d] The Family Care Center is a semiautonomous agency under the oversight of the Lexington-Fayette Urban County Government's Department of Social Services' Division of Family Services.

[e] This agency was formerly named Multi Resource Centers, Inc.

[f] This agency was formerly named Pittsburgh in Partnership with Parents.

program with all the elements of New Chance; all had to supplement their existing services and integrate them in a single program.

The program model, summarized in Table ES-2, shows that New Chance unfolded in two phases. At most sites, Phase I centered on education, career exposure, and a number of services falling under the general rubric of "personal development" (for example, parenting, family planning, and life skills). During this phase, services were delivered mostly at the program site—a "one-stop shopping" approach designed to facilitate participation. Typically, the program ran from 9 A.M. until 3 P.M. five days a week, with daily attendance at all classes expected. Local programs were intended to be small in size, enrolling 100 participants over 12 to 18 months and serving about 40 participants at any given time, in order to promote an intimate and personal environment in which participants and staff could establish close bonds.

Phase II services encompassed occupational skills training and work experience (both of which were generally off-site) and ultimately job placement assistance. Although college was not a formal part of the New Chance model, staff members at some sites encouraged participants to enroll in college, especially in two-year programs with a vocational focus.

Enrollees were permitted to remain in the program for 18 months, throughout which time case managers were expected to counsel them and monitor their progress. For as long as they remained active, participants also had access to child care at no cost to themselves; often, the care was provided at the program site.

As the preceding description suggests, New Chance services were directed primarily toward the young mothers and (in the form of child care and pediatric health care) toward their children. Local programs made efforts to reach out to the young women's parents and partners (for example, by inviting them to "graduation" ceremonies and other festivities), but the focus was on individual participants rather than on their extended families or the broader environments in which they lived.

Findings Regarding Research Sample Members

- **The New Chance sites recruited a diverse but generally very disadvantaged group of young mothers.**

Upon entry into the study, the young women averaged just under 19 years of age and, on average, had first given birth at age 16. Most (78 percent) were members of minority groups; fewer than one in ten had ever been married. About one third already had two or more children, and over half (53 percent) had a child under a year old. Indicative of their disaffection from school was the fact that 38 percent had dropped out before their first pregnancy and that applicants had typically been out of school for more than two years when they were randomly assigned to the experimental or control group. While over three quarters of sample members (79 percent) had some work experience, 63 percent had not worked at all in the 12 months before they applied to New Chance. Importantly, over half registered scores on a widely used scale indicating that they were at risk of clinical depression.

Table ES-2

The New Chance Model

Target Group

Mothers 16 to 22 years old who (1) had first given birth at age 19 or younger, (2) were receiving AFDC, (3) did not have a high school diploma or GED, and (4) were not pregnant when they entered the program

Program Structure and Services

Service Components:

Orientation

Phase I
Employment preparation components: Adult basic education, GED preparation, career exploration, pre-employment skills training
Components to enhance personal and child development: Life Skills and Opportunities curriculum, health education and health care services, family planning, adult survival skills training, parenting education, pediatric health services

Phase II
Employment preparation components: Occupational skills training, work internships, job placement assistance

Case management

Child care

Service Emphasis: Integration and reinforcement in each component of all program messages and skills

Service Structure: Sequential phases of program activities, relatively long duration (up to 18 months), high intensity, primarily on-site service delivery

Environment: Small, personal programs; warm and supportive, but demanding, atmosphere

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In short, the young women applying to New Chance had characteristics that for many would suggest difficult progress toward a GED and employment. They were also mostly adolescents, unsure of themselves and their goals. And they were responsible for the care of very young children.

Nonetheless, there was also considerable diversity within the research sample. Thus, while almost one quarter (24 percent) read at the 6th-grade level or below, 30 percent read at the 10th-grade level or above. And while 17 percent grew up in families that had always received welfare, 36 percent grew up in families that had never done so. These differences point to the possibility of different effects for different subgroups of the New Chance population.

- **During their stay in New Chance and afterwards, participants faced many barriers to steady participation and stable employment.**

Some of the problems that interfered with the young women's participation were ones experienced by working mothers in all income categories: children's illnesses (as well as their own) and breakdowns in child care arrangements. Other problems, however, were exacerbated by participants' poverty; for example, nearly half of an early group of enrollees were homeless or at high risk of homelessness during their stay in the program.

Case managers became aware of some problems over time, as they learned more about participants' lives or as these problems hindered attendance. Thus, for example, between 10 and 20 percent of the early cohort of experimental group women were known by the case managers to be physically abused by their partners, to use drugs or alcohol to such an extent that it interfered with their program participation (or to have partners or relatives who did so), or to be discouraged by important people in their lives from attending New Chance or otherwise advancing toward self-sufficiency.

- **Despite these problems, over time members of both research groups moved forward in many areas of their lives.**

When they entered the research sample, almost 94 percent of the sample members held neither a high school diploma nor a GED; by the 3½-year point, just under half (49.3 percent) had received one of these credentials. Employment rates also rose steadily throughout the follow-up period. In the year prior to entering the study, only 37 percent worked at all; during the last year of follow-up, in contrast, 52 percent were employed at some point. There was also substantial growth in average monthly earnings, both because of an increase in the proportion of sample members who were employed and because those who did work earned more. The rate of AFDC receipt remained high—nearly three quarters of the young women were receiving welfare at the 3½-year point. Nonetheless, this fraction represents a sizable drop from the 95 percent receiving assistance at baseline.

Time brought positive changes in the young women's personal lives as well. For one thing, they were doing more to plan their childbearing. Although the majority of young women experienced a repeat pregnancy and birth during the follow-up period, at the 3½-year point over half were using a reliable method of contraception. Also, there was a significant drop in the percentage of young mothers who were at risk of depression, although that proportion remained distressingly high (53 percent at baseline versus 44 percent at the 3½-year point).

Findings on Program Implementation, Participation, and Costs

- **The demonstration sites put in place all the early components and were, with only a few exceptions, able to offer the hours of each service prescribed by the program guidelines; some components, however, were easier to implement than others.**

The sites were all able to mount the early components of the New Chance model and to provide a relatively uniform treatment. Building the program infrastructure required a great deal of effort, however, especially given the multiple activities to be put in place in the compressed start-up period, which was only about six months long.

At all sites, education—both instruction in basic academic skills and GED preparation—was a central activity during Phase I, usually scheduled for about 12 to 15 hours a week. Parenting and life skills classes were each scheduled for about two hours weekly during this phase as well. Education proved to be one of the easiest components to implement, in part because experienced instructors were widely available and in part because enrollees were themselves interested in getting their GED certificates.

Other activities posed greater difficulties. The implementation of career exploration and preemployment skills instruction was slow and often unsystematic, because sites lacked experience with these components and ready-made curricula in these areas were unavailable. And because of time constraints, personal discomfort, or lack of expertise, case managers did not consistently use the individual counseling sessions to follow up on the young women's family planning practices, as prescribed by the program model.

- **Later activities—skills training, work internships, college and job placement, and ongoing case management—were more difficult to implement and were less uniform across sites.**

Phase II activities, mostly delivered off-site, were much more individualized than the earlier components. For each participant, staff had to find an activity in the community that was not only appropriate to her needs, interests, and abilities but also accessible and available when she was ready to enter it. Because skills training and employment resources varied from site to site, there were considerable differences among the sites in the way this phase of the program model was implemented. Moreover, because of the demands of their on-site caseloads, case managers were seldom able to maintain the biweekly contact with off-site participants that was specified in the program guidelines.

- **The quality of child care at the on-site day care centers was fairly good—higher than that typically provided by centers serving primarily low-**

income families, although below the level of care in facilities that have been found to improve children's developmental outcomes.

Regular on-site child care was provided to New Chance participants at 9 of the 16 sites; 2 additional sites offered child care on a temporary, drop-in basis. (Programs without on-site facilities helped participants with their child care arrangements, sometimes through linkages with nearby child care centers.) Information from staff at the on-site centers indicated that the New Chance child care centers generally met or exceeded experts' standards in terms of such structural characteristics as group size and child-to-staff ratios. Furthermore, observers who were trained to rate various aspects of child care visited 11 centers (4 of them off-site) and found that they were providing care that compared favorably with the care provided in centers serving low-income families, as reported in two major studies of child care centers. The observers rated the care as being of reasonably good quality. The quality of care, however, was not as high as that which characterizes child care programs that have been found to foster children's development.

- **In general, participation was much less intensive than had been planned, although members of the experimental group varied considerably in the regularity and duration of their program attendance.**

About 89 percent of experimental group members participated in some New Chance activity. (The remaining 11 percent dropped out between the time they were randomly assigned and the actual start of program activities.) On average, the young women participated for 296 hours in activities other than counseling, for which hours of participation were not counted, within 18 months after random assignment. This average conceals a great deal of variation, however; along with the 11 percent who did not participate at all, another 25 percent participated for 100 hours or fewer, while 22 percent registered more than 500 hours.

In part, low participation hours reflect erratic attendance, which was a serious problem at many sites, and in part they reflect early departures from the program. Although young mothers were permitted to stay in New Chance for 18 months, the average number of months (not necessarily continuous) they actually were active in the program was only 6.4—roughly one third of the maximum.

High absenteeism and early departures, taken together, made for a program treatment that was considerably more attenuated than planners had intended. In fact, on average, participants got only between 30 and 40 percent of the quantity of services planned for Phase I. The majority of enrollees who were unsuccessful in earning a GED while in New Chance (who constituted the majority of those in the experimental group) never moved on to Phase II activities at all, having dropped out of the program first. Consequently, only about one third of the members of the experimental group received the skills training that program planners envisioned as critical to their obtaining good jobs. Sites tried to improve participation in various ways—by stating requirements and expectations more clearly, by following up on absentees promptly, and by developing rewards for good attendance—but with inconsistent results.

- **Members of the experimental group received a greater quantity of services, as well as more varied services, than did their control group counterparts, and they also received them sooner.**

As Table ES-3 shows, a higher proportion of experimental than of control group members received each type of service; experimental group members also received a greater quantity of these services. For example, 84 percent of the experimental group members attended adult basic education or GED classes during the 3½-year follow-up period, and they averaged 26 weeks in these classes; 63 percent of the control group members participated in such classes, for an average of 16 weeks.

In accordance with the program's intent to deliver a wide array of services, those in the experimental group also received more kinds of services than those in the control group. The average experimental group member received five different services, while the average control group member received only three.

Finally, experimental group members received services sooner after random assignment than did control group members.

- **Levels of service receipt by control group members in the New Chance Demonstration greatly exceeded expectations and were higher than those found in previous demonstration programs for young mothers.**

Although it was expected that New Chance, as a voluntary program, would draw applicants who were motivated to take part in the kinds of activities New Chance offered, the level of service receipt among control group members was much higher than anticipated. Education- and employment-related services outside of New Chance were generally readily available in the communities where research sample members lived, and members of both research groups made extensive use of them. (Experimental group members mostly availed themselves of other service provider agencies after they had left New Chance.) Thus, while over the 3½ years 94 percent of the experimental group women participated in employment-related services—that is, education, skills training, or organized group job search activities that could be expected to provide access to the skills and credentials necessary for getting jobs—so did 85 percent of the women in the control group.

This level of service receipt by control group members can be put into perspective by comparing it with the findings of other studies. New Chance control group members received more services than control group members in several other demonstration programs enrolling disadvantaged young mothers and, indeed, received more services than did the experimental group members in other programs for this population that required participation as a condition of receiving welfare. This fact must be borne in mind when the impacts of the various demonstrations are compared.

- **Both experimental and control group members continued to participate in education and skills training throughout the follow-up period.**

Table ES-3

Selected Impacts of New Chance on Service Receipt Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Ever participated in[a] (%)			
Any education, skills training, or job club	94.5	85.9	8.6 ***
Adult basic education/GED preparation[b]	83.5	63.2	20.3 ***
Skills training	47.5	38.1	9.4 ***
Parenting classes	66.8	21.3	45.5 ***
Family planning classes	52.6	12.3	40.3 ***
Life skills classes	52.0	12.4	39.6 ***
Health education classes	50.0	11.1	38.9 ***
Average number of weeks in			
Any education, skills training, or job club	53.1	36.5	16.6 ***
Adult basic education/GED preparation[b]	26.2	16.3	9.9 ***
Skills training	13.4	10.1	3.3 ***
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The services listed are major components of the New Chance model. For control group members, services were obtained at programs or agencies other than New Chance. For experimental group members, the services were obtained either at New Chance or, if they were served by additional programs, elsewhere.

[b] The General Educational Development (GED) certificate is given to those who pass the GED test and is intended to signify knowledge of basic high school subjects.

Experimental group members who participated in education and skills training tended to do so earlier than control group members; nonetheless, some 30 percent of the members of both groups were attending an education program, and about 16 percent were attending a skills training program, at some point during the last year of the 3½-year follow-up period. In fact, the average sample member was enrolled in education or training for one quarter of the follow-up period.

This extended participation in education and training resulted in deferred entry into employment. Participation in education or training was the single most important reason that respondents to the 3½-year interview who were neither working nor looking for work gave for their absence from the labor force. One implication is that both experimental and control group members can be expected to work more as they move out of these activities into the labor market.

- **The cost of providing New Chance services to a young woman assigned to the program was approximately $9,000, with child care, recruitment, and case management services accounting for almost two thirds of the cost.**

The majority of New Chance expenditures were for child care and case management, services that were considered necessary to support participation in New Chance. By contrast, education, skills training, and other classes and workshops accounted for a much smaller share of the costs of implementing New Chance.

- **Comparing the total cost of all services provided to members of the experimental and control groups, the net cost of New Chance was estimated to range from $6,197 to $7,445, depending on the estimation method.**

Outside of New Chance, many members of the control group received services that were similar to those received by members of the experimental group and that involved substantial costs. Members of the experimental group also received services outside of the New Chance program. A range of possible net costs is presented because the costs of services to control group members, and of services provided to experimental group members outside of New Chance, could not be measured with the same level of precision as New Chance program costs. The cost of services provided to control group members is estimated to be between $5,555 and $9,024, depending on the method used. Estimates of the corresponding total cost per experimental group member (including New Chance and non–New Chance services) range from $13,000 to $15,221. As a result, estimates of the program's net cost range from $6,197 to $7,445. As with the New Chance program costs, most of the net costs were accounted for by the provision of child care and case management to New Chance participants, with education and training services accounting for less than a third of the net costs.

Program Impacts on Education and Training Credentials

- **Experimental group members were more likely than control group members to receive a GED and to earn college credits.**

As the program intended, New Chance raised experimental group members' GED attainment above that of control group members (see Table ES-4). At the 3½-year point, 45 percent of the experimental group members and 33 percent of their control group counterparts had attained this credential. The difference was statistically significant—that is, unlikely to have arisen by chance. (Statistically significant differences between outcomes for experimental and control group members are referred to as program *impacts* or *effects*.)

Although few members of either group attended high school, those in the control group were significantly more likely than those in the experimental group to have earned a high school diploma by the time of the 3½-year interview. At that point, 52 percent of the experimental group members held either a GED or a high school diploma, as did 44 percent of the control group members; this 8 percentage point difference, while statistically significant, was smaller than expected.

New Chance also had a small but significant effect on the proportion of young women who earned college credits toward an A.A. or B.A. degree. Fourteen percent of experimental group members and 11 percent of control group members reported having earned such credits.

- **Increased participation in education classes was associated with an increased rate of credential attainment.**

Additional analyses were conducted to explore the relationship between the quantity of education services received and the attainment of education credentials. These analyses controlled statistically for the likelihood that those who receive a high amount of services are different in many respects from those who receive a low amount. The results, while less definitive than the impact findings based on comparisons of outcomes for experimental and control group members, strongly indicate that receiving more than 18 weeks of adult basic education and GED classes was associated with a sizable increase in the proportion of sample members earning a GED credential.

- **Despite greater participation by experimental group members in skills training, members of the two research groups were equally likely to earn a trade license or certificate.**

About one quarter of the women in both groups had earned a trade license or certificate by the 3½-year interview. About one in six sample members (18 percent of the experimental group members and 16 percent of the control group members, a difference that is not statistically significant) had earned both a GED or high school diploma and a trade license and thus might be considered especially attractive to employers.

- **Despite experimental group members' higher rate of GED receipt, the program did not have an impact on educational achievement as measured by reading test scores.**

Reading scores on the Tests of Adult Basic Education (TABE) in the 1½ years following entry into the study rose from the 7.6 grade level to the 7.8 grade level for experimental group members and from the 7.7 to the 7.9 grade level for control group members. There was no

Table ES-4

Selected Impacts of New Chance on Credential Attainment and Educational Achievement at or Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Education credentials by end of month 42 (%)			
High school diploma or GED[a,b]	51.9	43.8	8.1 ***
GED	45.2	33.4	11.8 ***
High school diploma	6.9	10.4	-3.5 ***
Credits toward A.A. or B.A. degree	13.5	10.7	2.8 *
Trade certificate or license	25.2	24.7	0.5
Average reading score at 18-month follow-up (grade level equivalent)[c]	7.8	7.9	-0.1
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The percentages shown are for all sample members, including the 6 percent who had already achieved a high school diploma or GED when they applied to the program.

[b] The General Educational Development (GED) certificate is given to those who pass the GED test and is intended to signify knowledge of basic high school subjects.

[c] The test administered was the reading part of the Tests of Adult Basic Education (TABE), Survey Form, a 30-item test of reading vocabulary and reading comprehension. Sample sizes for this outcome are considerably smaller than those shown.

significant difference between the two groups in their reading levels at either baseline or follow-up, nor between the size of the gains registered by experimental and control group members.

Impacts on Living Arrangements

- **Changes in living arrangements were common among members of both research groups, but New Chance may have accelerated this process among members of the experimental group by helping some participants move out on their own when program staff had reason to believe a move was necessary; the result was greater instability in the living arrangements of members of the experimental group.**

Young adulthood is often a stage when living arrangements are in flux and when many people are testing alternatives to living with their parents. There is evidence that New Chance speeded up this process when program staff perceived participants to be in abusive or highly conflictual living arrangements and intervened.

Experimental group members moved an average of 4.0 times between the birth of their youngest child (that is, the youngest at the time of random assignment) and the 3½-year follow-up point, while control group members moved 3.7 times—a difference that, while small, was statistically significant (see Table ES-5). At the 1½-year follow-up interview, more experimental than control group members (23 versus 19 percent) reported living with a partner or husband, and fewer reported living with a parent or a grandparent (28 percent versus 35 percent). These differences had disappeared by the 3½-year interview, so that while control group members did not move away from parents as quickly as experimental group members they eventually did so.

At 3½ years, a somewhat greater proportion of experimental than control group members were living without any of their children (5 versus 3 percent, respectively); the reason for this finding is uncertain. Experimental group members were also more likely than women in the control group to report having had trouble finding a good place to live in the past 12 months, although large percentages of women in both groups (42 percent of the experimental group members and 38 percent of the control group members) experienced this problem.

Impacts on Fertility, Health, and Emotional Well-Being

- **Over time, the two groups had similar rates of pregnancies, births, and abortions.**

As Table ES-6 shows, about three quarters of the young mothers in both groups had another pregnancy during the follow-up period, and just over half had another baby. New Chance did not reduce the rate of pregnancies or childbearing. Indeed, women in the experimental group were more likely than women in the control group to be pregnant during 9 of the first 24 months after random assignment. (This increase in pregnancy rates appears to be related to the fact that at the 1½-year point, although not at 3½ years, women in the experimental group were more likely to be living

Table ES-5

Selected Impacts of New Chance on Living Arrangements at or Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Living arrangement at 42-month follow-up			
Living with parent or grandparent (%)	21.3	20.4	1.0
Living with husband or partner, but without parent or grandparent (%)	30.7	31.7	-1.0
Living with children only (%)	35.7	38.9	-3.2
Living in another arrangement (e.g., with friends, alone, in an institution) (%)	12.3	9.0	3.3 ***
Average number of times moved between birth of child and 42-month follow-up[a]	4.0	3.7	0.3 **
Has had trouble finding a good place to live in past 12 months (%)	41.9	37.5	4.4 *
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] This item pertains to the focal child, who was randomly selected from among other children already born at random assignment and was the focus of all child-related questions on the 3½-year survey.

Table ES-6

Selected Impacts of New Chance on Fertility-Related Behavior and Physical or Mental Health at or Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Fertility-related behavior			
Ever gave birth during months 1-42 (%)	54.7	55.3	-0.7
Ever became pregnant during months 1-42 (%)	75.2	72.8	2.3
Ever had an abortion during months 1-42 (%)	17.4	14.8	2.5
Sexually active, using contraception regularly at follow-up[a] (%)	41.4	44.0	-2.6
Physical and mental health at 42-month follow-up			
Personal health rated as very good or excellent (%)	52.9	51.7	1.2
Had no Medicaid or private insurance (%)	8.7	9.7	-1.1
Average score on CES-D (depression) Scale[b]	16.1	15.2	0.9 *
At risk of clinical depression (%)	44.6	42.5	2.1
Felt stressed much or all of the time in past month (%)	39.4	33.2	6.2 ***
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] A respondent who reported using contraception at each intercourse and/or who said that she always took a birth control pill when she was supposed to was considered to be using contraception regularly. Sample sizes for this outcome are smaller than those shown.

[b] The Center for Epidemiological Studies Depression (CES-D) scale is a widely used measure of depression; scores can range from zero to 60. Those with scores below 16 on the CES-D are not considered to be at risk of depression; those with scores of 16 and above are considered at risk.

with a partner.) By the 3½-year point, however, the cumulative pregnancy rates of the two groups did not differ significantly, nor did the cumulative rates of abortions and live births.

- **The New Chance findings are in line with those of other demonstration programs, which also have found it difficult to reduce the rate of subsequent pregnancies and births among teenage mothers.**

A majority of disadvantaged young mothers in several other research and demonstration programs, like those in New Chance, had a subsequent pregnancy within two years after baseline, and a sizable minority had a subsequent birth. None of these programs proved effective in reducing fertility. This finding suggests that many young mothers may not be strongly motivated to postpone childbearing or may actively want another child; it may also be that those who would like to delay childbearing are nonetheless subject to a wide range of pressures from partners, family members, and others—pressures over which program staff have little or no control.

- **Members of the two research groups exhibited similar patterns of contraceptive use at the 3½-year interview.**

At 3½ years, the two groups were similar in the proportions who reported that they were sexually abstinent (about 19 percent), sexually active and using contraception regularly (about 43 percent), or sexually active but not using contraception regularly (about 28 percent). At that point, over half the sample members were using a prescription method of birth control (birth control pills, Depo-Provera, or NORPLANT®) or had had a tubal ligation. The majority of women who reported having had a tubal ligation (13 percent of the sample) had had three or more children.

Most sample members were not fully protected against sexually transmitted diseases. Sixty percent of the women in both groups reported that they had had sex that was not protected against such diseases in the two months preceding the 3½-year interview.

- **New Chance did not affect participants' health status.**

Women in the experimental and control groups had comparable health-related outcomes at both the 1½-year and the 3½-year points. About half the women in both groups rated their health as very good or excellent, and experimental and control group members were equally likely to report smoking, using drugs, and drinking enough to get high. Fewer than 10 percent of sample members lacked health care coverage, which was generally provided through either Medicaid or private insurance.

- **At the 3½-year point, members of the experimental group scored higher on a measure of risk of clinical depression than did their control group counterparts; they were also more likely to report feeling stressed.**

As was noted earlier, the young women in the research sample were consistently at risk of depression. Using the Center for Epidemiological Studies Depression (CES-D) scale, which indicates risk of a clinical diagnosis of depression, 53 percent of all sample members were at such risk at the time of random assignment, and 43 percent remained at risk 3½ years later. Scores in the control group, however, improved significantly more than those in the experimental group, with the result that the average score for experimental group women was significantly higher than that for women in the control group at the 3½-year point.

In the 3½-year interview, women were also asked how much of the time in the previous month they felt highly stressed. Significantly more women in the experimental group (39 percent) than in the control group (33 percent) reported feeling stressed all or much of the time during the prior month.

Greater instability in living arrangements may help to explain the New Chance program's unexpected negative effects on enrollees' psychological well-being. It also seems plausible that the program raised expectations among participants that their lives would change; their depression scores may have improved less than those of women in the control group because of their failure to realize these expectations. Finally, as is discussed in a later section, mothers in the experimental group assessed their children's behavior less positively than did those in the control group; this fact, too, may be related to, or help account for, greater depression and stress on their part.

Impacts on Employment, Earnings, Welfare Receipt, and Family Income

- **Except during the six-month period following random assignment, employment rates for the two groups did not differ.**

As Table ES-7 shows, employment rates for both groups increased over time. As expected, women in the control group had higher rates of employment than those in the experimental group during the first six months after random assignment, when the latter were most likely to be active in New Chance. Thereafter, employment patterns did not differ. Similar proportions of both research groups (70 percent of experimental group members and 66 percent of control group members) were employed at some point during the follow-up period, and similar percentages (56 percent of the experimental group members and 55 percent of the control group members) held a full-time job—surprisingly high rates, given the age of the young women and of their children. Finally, similar proportions of both groups (28 percent of the experimental group members and 31 percent of their control group counterparts) were working at the time of the 3½-year follow-up interview.

The average duration of the first job was about six months. Among sample members who had been employed but were not working at follow-up, 64 percent of the respondents said they had resigned from their last job, 18 percent reported that the job was a temporary one that ended, 11 percent were laid off, and 6 percent were fired. Among those who had resigned, the three most commonly cited reasons for doing so were child care (accounting for 18 percent of those who resigned), pregnancy (14 percent), and problems getting along with supervisors and co-workers (14 percent).

- **New Chance did not produce increased earnings over the 3½-year follow-up period.**

During the first year and a half after random assignment, women in the experimental group,

Table ES-7

Selected Impacts of New Chance on Employment, Earnings, Welfare Receipt, and Family Income at or Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Ever employed (%)			
Months 1-6	15.1	20.4	-5.3 ***
Months 7-18	38.7	39.7	-1.0
Months 19-30	41.4	39.5	1.9
Months 31-42	53.3	50.5	2.8
Months 1-42	69.5	66.2	3.3
Employed at month 42 (%)	27.8	30.9	-3.1
Total earnings ($)			
Months 1-6	263	358	-95 **
Months 7-18	1,096	1,323	-227 *
Months 19-30	1,884	2,014	-130
Months 31-42	3,012	3,045	-33
Months 1-42	6,255	6,741	-486
Ever received AFDC (%)			
Months 1-6	96.2	94.4	1.7 **
Months 7-18	94.9	93.0	1.9 *
Months 19-30	91.2	90.5	0.7
Months 31-42	84.7	85.4	-0.7
Months 1-42	98.9	97.9	1.0 **
Receiving AFDC at month 42 (%)	75.4	73.5	2.0
Total monthly income (month 42)[a] (%)			
Less than $600	23.5	22.2	1.3
$601 - $900	29.9	31.1	-1.1
$901 - $1,500	23.2	23.1	0.1
More than $1,500	23.4	23.6	-0.3
Average income in month before 42-month interview[a] ($)	1,113	1,150	-36
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] Total income consists of AFDC, food stamps, and earnings (for the sample member and her husband or partner), and some other sources.

as expected, earned less than women in the control group; thereafter, however, total earnings were similar for the two groups. (For example, experimental group members earned $3,012 and control group members earned $3,045, on average, during the last year of the follow-up period.) Average hourly wages of sample members who worked were also nearly identical—$5.66 for women in the experimental group and $5.68 for women in the control group during the same period. So were their fringe benefits; roughly one quarter of those who worked had jobs that provided paid sick days, and one fifth had jobs offering a health plan or medical insurance.

It is worth noting that if young mothers worked at $5.67 an hour for 40 hours a week, 52 weeks a year, their earnings would still leave them more than $1,000 below the 1996 poverty guidelines of $12,980 for a family of three. Coupled with the lack of fringe benefits, such low earnings would leave most of these young mothers without a "safety net," a cash reserve for use in emergencies.

- **There is evidence that members of both research groups who received a GED or who participated in skills training and college had higher earnings than they would have had otherwise.**

As with the analysis of the relationship between amount of education and GED attainment, analyses of the relationships between earnings and either receipt of a GED or participation in training or college produce findings that are more uncertain than those grounded in experimental/control comparisons. Nonetheless, it appears that participants who obtained a GED or high school diploma had higher earnings than those who did not; this conclusion holds up when measured background differences between GED earners and non-earners are statistically controlled. (Interestingly, greater participation in adult basic education and GED classes by itself did not result in increased earnings unless participants actually obtained a credential.) The largest earnings increases, however, were realized by those taking part in skills training and college. This finding suggests that education credentials are important to earnings because of their "gatekeeper" function; that is, they are frequently a prerequisite for entry into college or programs that offer skills training.

- **New Chance had a minimal impact on welfare receipt.**

During the first 1½ years after random assignment, women in the experimental group were slightly more likely than women in the control group to receive welfare; thereafter, the two research groups had similar rates of AFDC receipt.

The proportion of sample members on the AFDC rolls declined over time. Forty-seven percent of both experimental and control group members left welfare at some point during the follow-up period, although fewer than half of these remained off welfare for 12 months or more. At the end of the follow-up period, about three quarters of the women in both groups were receiving AFDC.

- **There was a wide range of incomes in both experimental and control groups, but no experimental/control difference in the average amount of total family income sample members received.**

Sample members were asked about their income and the sources of that income in the month before the 3½-year interview. (This month was often, but not always, the same as the 42nd month of follow-up.) A higher percentage of women in the control group than women in the experimental group (26 versus 23 percent, respectively) reported receiving income from a husband's or partner's employment, while a slightly higher proportion of women in the experimental group than women in the control group (5 versus 3 percent) reported receiving Supplemental Security Income (SSI, federally assisted cash welfare for the disabled, aged, and blind). Otherwise, income sources were virtually identical for the two groups. Approximately one third of the members of both groups reported income from their own earnings, about 70 percent received AFDC, and some 78 percent received food stamps.

While the average income during the month before the 3½-year interview was $1,113 for experimental group members and $1,150 for control group members (a difference that was not statistically significant), there was a good deal of variation around this average. Approximately one quarter of the sample members fell into each of four monthly income categories: less than $600, between $601 and $900, between $901 and $1,500, and more than $1,500. Earnings of partners and spouses constituted a major income source for women in the last category.

Impacts on Parenting, Child Care, and Child Development

- **Overall, the children of experimental and control group members were being raised in similar home environments, although New Chance did produce positive impacts for those mothers who were not at risk of depression.**

At the time of the first follow-up interview, children in the experimental group were being raised in somewhat more favorable environments than were children of control group members, as indicated by a widely used scale known as the HOME.[4] This scale measures several aspects of the home environment that have been shown to be related to child development, including the degree of cognitive stimulation the environment provides, the cleanliness and safety of the environment, the mother's degree of emotional support in her interactions with her child, and the mother's use of harsh discipline. As Table ES-8 shows, at the 3½-year point the positive impact for the experimental group as a whole was no longer evident; experimental and control group members achieved similar HOME scores. Positive effects in the home environment persisted, however, among the subgroup of mothers in the experimental group who were not at risk of depression when they entered the research.

In retrospect, it seems likely that the rather modest number of parenting classes that participants received was inadequate to produce substantial impacts on parenting behavior,

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[4] A special study of parenting behavior carried out about 22 months after random assignment that relied upon direct observation of mother-child interactions rather than interview measures confirmed and extended these positive results for a selected (but statistically nonrepresentative) set of families at a set of New Chance sites. See Zaslow and Eldred (eds.), forthcoming.

Table ES-8

Selected Impacts of New Chance on Child-Related Outcomes at or Within 3½ Years After Random Assignment

Outcome	Experimentals	Controls	Difference
Average score on HOME scale[a] at month 42 for focal child[b]	100.1	100.0	0.1
Ever in a regular child care arrangement before age 1[b] (%)	48.4	41.0	7.4 ***
Average number of child care arrangements between baseline and 18-month follow-up	2.2	1.8	0.4 ***
Ever in a day care center or preschool by 42-month follow-up (%)	69.1	51.3	17.8 ***
Child's standard score on School Readiness subscale of Bracken Basic Concept Scale (BBCS) at 42-month follow-up[c]	6.6	6.9	-0.2
Child's standard score on Behavior Problems Index (BPI) at 42-month follow-up, maternal report[d]	110.0	108.5	1.5 **
Child's score on Positive Behavior Scale (PBS) at 42-month follow-up, maternal report[e]	192.1	197.3	-5.3 ***
Sample size	1,401	678	

NOTES: Calculations for this table used data for all sample members, including those who had values of zero for outcomes and experimental group members who did not participate in New Chance. For some of the outcomes, sample sizes may be slightly smaller than those shown due to missing or unusable responses in some sample members' questionnaires.

Rounding may cause slight discrepancies in sums and differences.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Outcomes in this table pertain to the focal child, who was randomly selected from among other children already born at random assignment and was the focus of all child-related questions on the 3½-year survey.

[a] A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

[b] Regular child care arrangement includes kindergarten or elementary school, extended day program, summer program or day camp, Head Start, day care center, nursery school, preschool, and grandparent, child's father, or other relative.

[c] The BBCS is a measure of receptive language that assesses the mastery of basic concepts; the School Readiness Component consists of five subtests of the BBCS: colors, letter identification, numbers, comparisons, and shapes. The scores shown are standard scores on a scale that ranges from 1 to 19; a standard score of 6.9 corresponds to about the 15th percentile nationally.

[d] The BPI is a widely employed scale for describing the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[e] The PBS is a 25-item scale developed for this study, with many items adapted from the Block and Block California Child Q Set. Scores for the total scale could range from zero (least favorable score) to 250 (most favorable score). The subscales were developed on the basis of a factor analysis.

especially for those mothers at risk of clinical depression. It may be that these young mothers need more intensive parenting services or an approach that combines mental health services with parenting education. Also, to the extent that the quality of the home environment is shaped by income and other economic factors, the absence of differences between experimental and control group members in these areas may also help to explain the lack of impacts on the home environment measure.

- **During the early months of follow-up, the children of experimental group members and those of control group members were exposed to very different child care experiences.**

Child care in the New Chance Demonstration was intended both to facilitate the young mothers' participation and to promote the development of their children. As expected, there were marked differences between the child care experiences of experimental and control group children during the first part of the follow-up period. Although most children in the control group (85 percent) were cared for by someone other than their mothers during the first year and a half of follow-up, they were in such care for shorter periods of time and were most likely to be cared for by relatives. In contrast, the children of women in the experimental group were most likely to have attended a day care center or preschool (64 percent of the children chosen as the focus of interview questions for the experimental group versus 31 percent of their control group counterparts) and received care for longer periods of time; they were also more likely to have experienced nonmaternal care by the time they were a year old.

Finally, during the early part of the follow-up period (that is, between random assignment and the 1½-year interview), the children of women in the experimental group experienced a greater number of different child care arrangements: 2.9 different arrangements, as compared with 2.6 arrangements for children of women in the control group, a statistically significant difference.

Between the 1½-year and the 3½-year interviews, children in the two groups had similar child care experiences. Because of pronounced differences during the first year and a half of follow-up, however, at the 3½-year point the proportion of children of control group members who had spent time in a child care center was still considerably smaller than the proportion of experimental group members' children who had done so (51 percent versus 69 percent, respectively).

- **Mothers in the experimental group reported a significantly greater amount of parenting-related stress than did mothers in the control group.**

A measure of parenting stress was included in both follow-up survey interviews. The two groups of women had similar overall scores at the time of the 1½-year interview, although women in the control group were more likely at that point to have scores reflecting dislike of the parenting role. The situation was different at the second follow-up, however, when mothers in the experimental group registered more parenting-related stress overall than did their control group counterparts. At the 3½-year point, too, experimental group mothers reported higher levels of aggravation in relation to their children than did control group mothers.

- **Children of experimental and control group members had similar low scores on a measure of cognitive development.**

New Chance did not have an effect on children's cognitive development, as measured by a test that assesses the child's mastery of basic concepts such as colors, letter identification, shapes, and comparisons. Average scores were similar for the children of members of the two groups. They were also low; the average child in both groups was at about the 15th percentile nationally.

- **Women in the experimental group rated their children as having more behavior problems than did women in the control group. This impact was concentrated among young women who were at risk of depression at baseline.**

Women in the experimental group reported significantly more behavior problems on the part of their children than did their control group counterparts; they also rated their children significantly lower on a scale of positive behavior. These negative impacts were concentrated among women who were at risk of depression at baseline and were registered for both boys and girls who were older than five years old at the 3½-year follow-up, as well as for boys who were under age five. Teachers' ratings of the academic proficiency and behavior of sample members' children provide partial confirmation of the mothers' assessments; the teachers rated the preschool- and school-age daughters (although not the sons) of mothers in the experimental group as presenting more behavior management problems than the daughters of control group women. (The teachers gave similar ratings to the children in the two groups in the area of academic performance.)

A substantial body of literature suggests that depression interferes with good parenting behavior and that inattentive or inconsistent parenting, in turn, leads to behavior problems among children. It seems plausible, therefore, that the greater degree of depression and parenting stress registered by women in the experimental group may account in part for the greater behavioral difficulties reportedly exhibited by their children. The fact that children of mothers in the experimental group experienced more disruption in their early child care experiences than did children of control group mothers may also help to explain the unexpected negative impacts of New Chance in this area.

Findings on Sites and Subgroups

- **No site stands out as having done markedly better than the others.**

The New Chance sites differed considerably in specific aspects of program implementation (for example, the quality of particular services, whether or not the site provided on-site child care, and the size of the staff) as well as in the environments in which they were located. Therefore, the evaluation sought to determine whether the program was more effective in some locations than in others. The results did not suggest that any particular site, or any group of sites, had more or less favorable impacts than other sites across a range of outcome areas, even when differences in the populations they served were taken into account.

- **Young women who had been out of school longer and who were at especially high risk of depression when they entered New Chance experienced adverse outcomes in a number of areas, as did their children.**

Previous studies have often found programs to be particularly effective or ineffective for specific subgroups of the research sample—that is, groups of sample members defined on the basis of their characteristics at the time they entered the research sample. Analysis of the findings for subgroups defined in this way does not suggest that any group especially benefited from New Chance.

At the same time, there is evidence that the program had negative effects for two groups of women in the experimental group who at baseline were more disadvantaged than other program enrollees—and negative effects for their children as well. First, women in the experimental group who at random assignment had been out of school for more than two years registered greater risk of depression at the 3½-year point than comparable women in the control group; they also reported higher levels of parenting stress, and they rated their children as exhibiting more behavior problems. Second, experimental group women who were at high risk of depression at random assignment stayed at a higher risk of depression than their control group counterparts at the 3½-year point and also reported a higher degree of parenting stress; their children demonstrated lower cognitive functioning and were rated by their mothers as having more behavior problems.

These findings suggest that young women at higher risk of depression and young women who have been out of school for an extended period need a very different treatment from the one provided by New Chance, especially because their children appear to be particularly at risk of negative outcomes. This possibility is discussed at greater length below.

Implications for Program Operations

The New Chance findings indicate that intensive receipt of education and training services made for better outcomes. Since services are important, the key issue is how to ensure better service delivery and take-up. The key features of New Chance—comprehensive services delivered on site and supported by intensive (and relatively costly) case management—did not ensure a level of service receipt that resulted in better outcomes for experimental group members and their children than for their counterparts in the control group.

It is reasonable to speculate whether New Chance would have been more effective had it been mandatory. Program coordinators, when interviewed, held differing opinions on the topic. The records of mandatory interventions for teenage mothers who were not in school at the outset of the intervention have not been promising; such interventions have also been ineffective in increasing self-sufficiency for this population.

The New Chance experience does offer clues about some measures that may promote more effective service delivery:

- **Conserving resources to ensure consistent follow-up of enrollees who are in off-**

site activities or are employed. Programs may wish to make one or more staff members specifically responsible for following up with and helping to resolve problems confronting participants in off-site activities, including employment.

- **Improving family planning services.** The unsuccessful record of New Chance and other demonstration programs in reducing rates of repeat pregnancy and childbearing among young women who are already mothers suggests that programs face a daunting challenge in achieving behavior change in this area. Programs would do well, however, to make available and encourage the use of longer-acting contraceptives, such as NORPLANT® and Depo-Provera.

- **Responding to mental health and other personal problems.** Program staff need to be aware of the high risk of depression in this population, of the debilitating effects of this mental health problem, and of resources for treatment, including medication when appropriate. They also should be familiar with community resources for treating substance abuse, helping domestic violence victims, and dealing with other problems.

- **Ensuring continuity of child care.** Frequent changes in child care arrangements are likely to have harmful effects on children. Program staff should help participants make child care arrangements that will be flexible enough to accommodate the mothers' needs as they move through various phases of the program and into subsequent employment.

It is worth noting that not all these measures entail additional resources; they might be put in place, rather, by redirecting resources toward somewhat different goals and means.

At the same time, it is worth experimenting with (or further refining) other, very different program models. While it is beyond the scope of this report to spell out these models, they might include further expansion of home visitor programs (see Olds, 1988), approaches that try to do only one or two things (such as education or parenting) but to do them intensely and extremely well, approaches that emphasize youth development and empowerment, and approaches that reach young mothers in the context of broader community development efforts.

Finally, while it may be politically unpopular or even infeasible to not *require* young mothers on welfare to do anything outside the home, it appears worthwhile, from a knowledge development standpoint at least, to test the notion that society's resources would be better expended on these women once they have attained a greater degree of personal stability and maturity. In the meantime, attention could focus on supporting the growth and development of their children. This issue, of course, reaches beyond the implications for program operations and into the arena of policy.

Implications for Policy

The New Chance experience provides few definitive answers about what should be done. It does, however, raise critical questions about the directions and consequences of public policy.

Implications for Welfare Policy. The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 gives states extraordinary leeway to design their own welfare programs within broad guidelines established by the law. New Chance was put in place in an environment quite different from the one that is likely to exist once the provisions of the new law are fully implemented. In particular, research sample members were not subject to time limits on their receipt of welfare; in contrast, the Act prohibits states from using federal funds to provide assistance grants to most families that have been on welfare for five years, and states can set much lower time limits if they choose. Despite such differences, the New Chance results have important implications for the design and implementation of the new state initiatives.

First, the 1996 legislation requires young mothers on welfare who are under age 19 to attend high school or an alternative program if they do not already have a diploma or a GED. In fact, high percentages of both experimental and control group members in New Chance did attend education classes. The problem was less their lack of initial effort than the lack of consistent and continuous participation, caused in part by lack of motivation but in part, too, by homelessness, domestic violence, child care and transportation problems, illnesses, and other problems not within participants' (or a program's) control. Policy makers will need to consider what supports young mothers need in order to participate regularly, as well as what circumstances warrant deferrals or exemptions from required activities.

The cost of child care is another factor policy makers will need to bear in mind in imposing participation requirements on young mothers. The data suggest that if welfare-to-work programs make a full effort to engage young mothers, providing care for their children would be expensive, given the large proportion of young mothers with children under age one and the high cost of infant care.

In a related vein, the findings suggest that policy makers will need to decide how to respond when former recipients lose their jobs. Although rates of job-holding in the New Chance sample were surprisingly high, so too were rates of rapid job loss, occasioned by pregnancy, the lack of good child care, conflicts with supervisors, transportation problems, and other factors. If high rates of job turnover remain the norm in the future, policy makers will need to weigh possible responses to this situation. One possibility would be to implement initiatives designed to help former recipients get new jobs as quickly as possible; another would be to have welfare case managers follow up on former recipients who move into employment to try to identify and deal with problems that can ultimately result in job loss.

With regard to time limits themselves, the territory is virtually uncharted. Time limits may lend greater urgency to recipients' efforts to attain self-sufficiency. The fact that three quarters of New Chance experimental and control group members remained on welfare after three and one half years, however, raises serious doubts about the ability of a large proportion of young mothers to attain self-sufficiency within a short time. The findings suggest that the shorter the time limits, the greater the number of recipients who will continue to need assistance once the time limit is reached—an important consideration when only 20 percent of the caseload can be exempted from the time limit.

Under the Personal Responsibility and Work Opportunity Reconciliation Act, states have the option of denying benefits for additional children born after mothers have begun to receive assistance. The results of New Chance and other demonstrations suggest the difficulty of modifying fertility-related behaviors. Whether "family caps" would succeed in this regard is very much an open question.

Indeed, the vision that appears to underlie the new legislation is that welfare recipients will act out of economic rationality—that they will exercise free choice and respond appropriately to the financial incentives built into the new laws and regulations. The New Chance data suggest that this is an overly simple view. While some young women may be prompted to move more quickly toward self-sufficiency, others are likely to be held back by depression (especially if it is undiagnosed and untreated), by the lack of supportive figures in their lives, and by other factors that constrain both choice and opportunity. In this event, they may be left with neither welfare nor work. If so, their children will encounter the negative developmental consequences of growing up in even deeper poverty than they normally experience on welfare.

Interventions Beyond the Welfare System. The New Chance results also indicate the need for public policies that move beyond the scope of the welfare system to enhance young mothers' efforts to become self-sufficient. The findings suggest that while they are still young, many mothers will not be able to find jobs that enable them to escape poverty; nor will the jobs they do find offer health insurance and the other fringe benefits that constitute a "safety net" that keeps people from slipping back into acute need. Whether or not marriage is a desirable solution, the experience of research sample members suggests that it is an undependable one, in part because disadvantaged men often face the same unstable labor market prospects as their female counterparts.

These realities suggest the need for income support policies outside the welfare system: income supplementation (for example, through the Earned Income Tax Credit for low-income households), extended Medicaid or another form of health insurance, and low-cost child care. Such policies shore up earnings and reward work effort; they also have the potential to lift families out of poverty.

The difficulty of effecting change in the behavior of young mothers—especially those who are school dropouts—through the provision of services above and beyond those that are already available suggests that policy makers may want to direct more attention toward other, related concerns. First, more consideration needs to be given to identifying and promoting effective strategies for delaying first pregnancies among teens. A second priority should be to assist the development of children growing up in poverty. The fact that children of New Chance sample members had preschool readiness scores placing them at only the 15th percentile nationwide suggests that, without intervention, these children may be prime candidates for poor academic performance, school dropout, premature parenthood, and unemployment.

Finally, it seems important to test public policy initiatives that are focused less on remediating the skills of individuals than on increasing economic opportunity more generally. Early childbearing and negative developmental prognoses for children arise in a context forged by poverty. Interventions that are focused on individuals often deal with the low skill levels and motivational issues that contribute to poverty. They do not, however, address the larger social forces that create poverty: the disappearance of decent-paying jobs for relatively low-skilled people, the special shortage of such jobs in low-income communities, the continuing effects of discrimination, the social isolation and lack of support in the workplace. It may be that programs focused on individuals would be more effective if they were embedded in, or accompanied by, more ambitious initiatives that seek change on the societal as well as the individual level.

Chapter 1

Introduction

I. The New Chance Demonstration: An Overview

One of the most important and hotly debated social issues in recent years has been how to forestall the long-term poverty and receipt of public assistance that are frequently associated with early childbearing and that often have negative developmental consequences for the children of teenage mothers. Passage in 1996 of federal welfare legislation has placed the issue in a new context and imbued it with a new urgency.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 is described in greater detail later in this chapter and in Chapter 9. In brief, it replaced the Aid to Families with Dependent Children (AFDC) program with cash block grants to states. Its provisions include lifetime limits on federally assisted welfare payments, work or training requirements, mandatory school attendance for teenagers who lack a high school credential, residence requirements for unmarried minor mothers, and (at the states' options, "family caps" denying additional benefits to children born after the mother goes on welfare.[1]

How these young mothers and their children will fare in this new environment is an open question, but there is a growing body of research that illuminates both the challenges of and potential directions for moving these families toward self-sufficiency. This is the final report on one such effort. It presents findings on the impacts of a national research and demonstration program, New Chance, which was designed to test an intervention to improve the economic status and general life prospects of young welfare mothers and their children. The mothers, aged 16 to 22 (and averaging 18.8 years old) when they entered the demonstration, had given birth as teenagers; almost all had left high school before graduating, and they participated in the program voluntarily. While the young mothers in New Chance were notably diverse in the personal characteristics and social supports they brought to the program, as a group they were all placed at high risk of long-term welfare receipt by their poverty, failure to have completed high school, and (in most cases) unmarried status.

New Chance was, in part, a human capital development program; it sought to help participants become self-sufficient by increasing their academic and vocational skills so that, over time, they could find and keep jobs offering opportunities for advancement and reduce their receipt of public assistance. It also sought to help participants acquire the motivation, knowledge, and skills to delay further childbearing, become better parents, and improve the quality of their decision-making and their communications with family, friends, and the wider world. Finally, the New Chance model was explicitly two-generational in its approach, seeking to enhance the cognitive, social, and physical development of participants' children. The wide range of the program's objectives make the New Chance findings of interest to a broad audience of policymakers, program operators, researchers, funders, and others concerned with early childbearing.

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[1] Included in the Act are changes in the Food Stamp program, the Supplemental Security Income program, child welfare services, and other programs that could greatly affect the lives of poor families. Such changes are beyond the scope of this report, however.

New Chance operated in 16 communities in 10 states across the country. As shown in Table 1.1, the 16 local New Chance programs (also referred to in this report as the research "sites") were operated by a diverse group of program sponsors including community service organizations, schools and school districts, a community college, a Private Industry Council (the nonprofit entity that administers funding under the federal Job Training Partnership Act, JTPA), and an agency overseen by the county government; one program (Portland, Oregon) represented a unique collaboration between a school district and the Job Corps.[2] The program model called for participation in a comprehensive array of services designed to address the young women's multiple roles as students, future workers, mothers, family members, and partners. Like other human capital development interventions, New Chance was primarily focused on improving the well-being of individual participants and their children, not on changing the social, economic, or political environments in which they lived. The local programs essentially built New Chance upon a foundation of existing services; as participants in the demonstration, they received $300,000 per site over a three-year period from state sources and demonstration funders to supplement these services and fill in any gaps.

The evaluation of New Chance includes three major components. The *process analysis* (also known as the *implementation analysis*) describes the New Chance population, the program structure and services, and the way the program was put in place at the various sites. It also analyzes patterns of program participation and retention for members of the experimental group. The *impact analysis* focuses on outcomes in the areas of educational attainment, family life, physical and emotional health, employment and earnings, welfare receipt, and child development. The *cost analysis* ascertains the gross and net costs of the program as a whole and of specific components and particular sites.

Between late 1989 and mid-1992, young women eligible for the program were randomly assigned to one of two groups. The 1,553 young women assigned to the *experimental* group were allowed to enroll in New Chance.[3] The 769 members of the *control* group were denied access to

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[2] Twelve of these sites are still operating a program like New Chance as of this writing. The Chula Vista, Bronx, Harlem, and Inglewood New Chance programs closed because of funding difficulties. These closings did not materially affect the program experiences of the young women at these sites for whom data are presented in this report. The report has been written mostly in the past tense, however, because it describes program structures and activities as they existed or occurred during the period from late 1989 until mid-1993.

[3] All program applicants filled out a New Chance Enrollment Form prior to being randomly assigned to the experimental group or the control group, but only those assigned to the experimental group were actually allowed to enroll in the New Chance program. Thus, the terms *enrollees* and *experimentals* refer to the same group—those young women who were given access to New Chance services—and are used interchangeably in this report. As is noted later in the report, most but not all of the young women who enrolled in New Chance actually received program services. The term *sample members* refers to all the young women assigned to the experimental and control groups, whether or not the experimental group members actually participated in program services.

Table 1.1

The New Chance Program Operators

Location	Program Operator	Type of Organization	Prior Emphasis
California (Chula Vista)	Del Rey Center, Sweetwater Union High School District[a]	Adult school	Adult education
California (Inglewood)	Southern California Youth and Family Center[a]	Community service organization	Counseling, health services
California (San Jose)	Independence Adult Center, East Side Union High School District	Adult school	Adult education
Colorado (Denver)	Technical Education Center–North Campus, Community College of Denver	Community college	Adult education, occupational skills training
Florida (Jacksonville)	The Bridge of Northeast Florida[b]	Community service organization	Family planning, health services, tutoring
Illinois (Chicago Heights)	Aunt Martha's Youth Service Center, Inc.[c]	Community service organization	Counseling, adult education, employment preparation, health services
Kentucky (Lexington)	The Family Care Center[d]	Agency overseen by county government	Prevention and treatment of child abuse and neglect
Michigan (Detroit)	Development Centers, Inc., Community Mental Health Center	Community service organization	Mental health services
Minnesota (Minneapolis)	RESOURCE, Inc.[e]	Community service organization	Occupational skills training, employment preparation
New York (Bronx)	National Puerto Rican Forum, Inc.[a]	Community service organization	Adult education, English as a Second Language, occupational skills training
New York (Harlem)	Mid-Manhattan Adult Learning Center, Office of Adult and Continuing Education, New York City Board of Education[a]	Adult school	Adult basic education, GED preparation, occupational skills training
Oregon (Portland)	PIVOT–New Chance Program, Portland Public Schools	School/Job Corps	K-12 and adult education
Oregon (Salem)	Teen Parent Program, The YWCA of Salem	Community service organization	Recreation, adult education, child care, counseling, health services
Pennsylvania (Allentown)	Expectant and Parenting Youth Program, Private Industry Council of Lehigh Valley	Private Industry Council	Adult education, life skills, personal development
Pennsylvania (Philadelphia)	Lutheran Social Mission Society/Lutheran Settlement House Women's Program	Community service organization	Adult education, life skills
Pennsylvania (Pittsburgh)	Young Mothers Program,[f] The Hill House Association	Community service organization	Comprehensive services for teenage parents

NOTES: [a] A New Chance program is no longer in operation at this site.

[b] This agency was formerly named Family Health Services.

[c] This agency is now located in Park Forest, Illinois.

[d] The Family Care Center is a semiautonomous agency under the oversight of the Lexington-Fayette Urban County Government's Department of Social Services' Division of Family Services.

[e] This agency was formerly named Multi Resource Centers, Inc.

[f] This agency was formerly named Pittsburgh in Partnership with Parents.

New Chance but were given a list of other programs and services available in their communities in which they were free to participate. Thus, the experiences of the controls were intended to reflect what is likely to have happened without New Chance.[4] At 18 and 42 months after random assignment, survey interviews were conducted to collect comparable data from members of both groups. The 18-month interview was designed to capture the relatively short-term impacts of program participation; the 42-month interview permitted examination of the program's longer-term effects on the mothers and also included an assessment of the sample members' children, to determine whether the program affected their cognitive and emotional development. At the 42-month follow-up, the young women were 22.4 years old on average. This report, based principally on data collected through these interviews, examines the program's impacts—that is, the differences between the outcomes for experimentals and those for controls—at both points in time.

The New Chance Demonstration has been funded by a broad consortium whose members are listed in Table 1.2. The program model and demonstration were developed by the Manpower Demonstration Research Corporation (MDRC), a private nonprofit organization that develops and tests initiatives to improve the well-being and self-sufficiency of poor people. The program model was based on consultations with experts in the field (including youth program operators, welfare administrators, and academicians) as well as a review of the literature. The preliminary model was refined after a six-site pilot test, which lasted more than a year, indicated the basic feasibility of the approach, and suggested some encouraging results. MDRC designed and carried out the research agenda, provided initial training and ongoing technical assistance to the demonstration sites, and monitored their compliance with the program model and the research requirements.

This is the fourth and final report on the demonstration. The first, a report on program implementation, described program start-up and contained early findings on enrollees' characteristics and participation.[5] The second was a monograph based on in-depth interviews with 50 young women two and a half years (on average) after they had left New Chance; it explored their life circumstances and activities during the post-program period.[6] The third examined impacts at 18 months after the young mothers had entered the research sample.[7]

The earlier reports pointed to several key findings that resound through this report as well:

- Most program components were put in place as planned, but some program elements and activities were implemented more fully than others.

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[4] Allowing programs to provide controls with a list of service alternatives was deemed essential to win sites' compliance with the random assignment process. The extent to which these lists may have familiarized the controls with other service options, facilitated their access to these services, and resulted in a greater degree of service receipt is unclear.

[5] Quint, Fink, and Rowser, 1991.

[6] Quint and Musick, 1994.

[7] Quint et al., 1994.

Table 1.2

Funders of the New Chance Demonstration

U.S. Department of Labor
Ford Foundation
W. K. Kellogg Foundation
DeWitt Wallace-Reader's Digest Fund
Meyer Memorial Trust
The UPS Foundation
Charles Stewart Mott Foundation
The Pew Charitable Trusts
Stuart Foundations
William T. Grant Foundation
The Skillman Foundation
The David and Lucile Packard Foundation
AT&T Foundation
The Bush Foundation
Foundation for Child Development
Exxon Corporation
The Chase Manhattan Bank, N.A.
Koret Foundation
ARCO Foundation
GE Foundation
National Commission for Employment Policy
Mary Reynolds Babcock Foundation
The Allstate Foundation
Neighborhood Reinvestment Corporation
Honeywell Foundation
The Pillsbury Company Foundation
Kaiser Permanente
Anonymous Funder

- The majority of young mothers in New Chance faced one or more serious personal or situational problems (for example, conflicts with parents and partners, unstable living arrangements, substance abuse) that interfered with their ability to participate in New Chance and to realize the program's goals.

- In spite of these difficulties, many young women exhibited great resiliency and continued to advance.

- The young women's lives were frequently changing as they moved in and out of programs, jobs, and relationships, and their active engagement in school, training, or work was often interrupted by periods during which they were involved in none of these activities.

- Young women in the experimental group received more services than those in the control group during the initial 18 months of follow-up, but participation was inconsistent, and the program was not as intensive in practice as had been planned.

- Young women in the control group, too, participated in various activities--especially adult education and literacy programs—in unexpectedly large numbers, so that the difference in the amount of services received by experimentals and controls was much smaller than expected.

This last point merits some elaboration. The extent of service receipt by members of the control group is an important factor in explaining impacts (or their absence) in any evaluation. Service receipt by controls is especially critical in evaluations of voluntary programs in locations where alternative services are widely available, because those who voluntarily apply for a program are presumably more likely to want the services offered and to seek them elsewhere if they are denied admission to that program.

Thus, the fact that New Chance served volunteers means that if, at the outset, members of the experimental group were able and motivated to receive the kinds of services New Chance offered, so were their control group counterparts. This is not a fault of the random assignment research design; members of any comparison group that was well-matched to the New Chance experimental group could be expected to receive many of the same services as program enrollees.

While program planners anticipated that control group members in the New Chance Demonstration would receive services, they did not expect their level of service receipt to be as high as it turned out to be. The fact that the control group received many of the same services as did the experimental group means that the evaluation does not test the value of the individual services New Chance offered, as would be the case if the control group had been relatively unserved. Instead, the research tests the value of a packaging strategy that was expected to result in a service increment for members of the experimental group well above and beyond what controls would receive. With respect to some services, however, this increment was not attained.

The 18- and 42-month findings indicate that both experimentals and controls made considerable progress over time, but that New Chance did not abet that progress very much. The program had a positive and statistically significant impact on receipt of a General Educational Development (GED) certificate and of college credits. It did not, however, produce the desired behaviors in the areas of fertility regulation, employment, or welfare receipt. Further, the effects of New Chance on participants' emotional well-being and on the development of their children were, in some cases, just the opposite of what was intended; that is, outcomes for experimentals were somewhat worse than those for controls. The report aims to explain, as well as to describe, these results insofar as possible.

These findings inevitably raise questions about the New Chance service package—specifically, about its intensity, context, and timing. But they do not imply that services are not helpful, or that they should be denied, to poor young women and their children. Indeed, a recurrent question addressed in this report is whether young women in both research groups who got services did better than those who did not. Although the question cannot be answered as definitively as questions concerning program impacts, the best available information indicates that some services indeed made a difference, while others did not.

This chapter sets the stage for the remainder of the report. The next two sections examine the issues associated with adolescent childbearing and the way in which welfare policy seeks to address these issues. The New Chance program model is then described in greater detail and the experiences of other programs for young mothers are reviewed, to establish a context in which the findings of this report can be understood. The chapter concludes with an overview of the rest of the report.

II. Adolescent Childbearing: The Issues

The past two decades have witnessed marked changes in the prevalence and patterns of teenage childbearing in the United States. The birthrate dropped from 68 births per thousand women aged 15 to 19 in 1970 to 50 such births in 1986 (Moore, 1993), a decline generally attributed to the legalization of abortion and to improved contraceptive use among teens (U.S. General Accounting Office, 1986). Between 1986 and 1991, however, birthrates rose markedly, to 62 births per thousand teenage women in 1991. In 1992 and 1993, birthrates fell slightly, so that in 1993 (the most recent year for which national data are available), there were 60 births per thousand women aged 15 to 19; it is too early to tell whether this marks the beginning of a longer-term downward trend (Moore, 1996).

Thus, the rate of births to teenagers is lower today than it was 25 years ago. The percentage of births to unmarried teens, however, has climbed dramatically. In 1970, only 30 percent of births to teenagers were to unmarried women. By 1993, 72 percent of the half-million births to mothers aged 19 and under occurred outside of marriage (Moore, 1996). It is important to note that this increase in out-of-wedlock births, far from being confined to teens, appears to be part of a much larger trend. In fact, the increase in out-of-wedlock childbearing has been greater among older women than among teens. Moreover, while early out-of-wedlock childbearing has historically been more common among blacks than among whites—a pattern that can be traced to the slavery period (Franklin, 1997)—the *increase* in unmarried childbearing has been greater among white teens than among blacks.[8]

Finally, with contraceptives more widely available and abortion now legal, it seems likely that teenagers who give birth today are different from their earlier counterparts. They may want children more, or they may be more disadvantaged, economically and otherwise; and if they perceive themselves as having very limited futures in any case, they may see fewer drawbacks to early childbearing.[9]

There is considerable evidence that teenage childbearing is associated with a host of negative life outcomes. Although there is controversy over the extent to which these outcomes are *caused* by an early birth (Furstenberg, 1991, Geronimus and Korenman, 1992, 1993; Hoffman, Foster, and Furstenberg, 1993; Hotz, McElroy, and Sanders, 1997)—many teenagers who become mothers would have been poor later on even if they had deferred childbearing—there is reason to think that having a baby compounds the disadvantage, reducing the likelihood that the young mother will ever obtain a high school diploma.[10] This is especially troubling at a time when, as a result of major changes in the labor market, postsecondary education or training is increasingly required for jobs that pay a living wage.

Early childbearing greatly increases the risk of leaving high school without a diploma (Upchurch, Astone, and McCarthy, 1990), and pregnancy is a major reason girls give for school dropout (Ekstrom et al., 1986). Nearly half the women who first gave birth at age 17 or younger during the early 1980s failed to complete high school, compared with under 10 percent of those who postponed childbearing until their early twenties (Upchurch and McCarthy, 1990); young mothers who dropped out before becoming pregnant were especially unlikely to complete their education (Upchurch, 1988). Other analyses indicate that, after controlling for a wide range of personal and community characteristics, having a child before age 20 reduces educational attainment by almost three years among whites, blacks, and Hispanics (Klepinger, Lundberg, and Plotnick, 1995).

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[8] Between 1980 and 1991, the rate of births to unmarried women aged 15 to 19 rose 21.6 percent (from 89.2 to 108.5 births per thousand unmarried women) for blacks and 102.5 percent for whites (from 16.2 to 32.8 births per thousand unmarried women). See U.S. Congress, 1994, Table G-3, p. 1110.

[9] It should also be noted that certain changes in the social context of teenage childbearing over recent decades might be expected to result in improved outcomes for young mothers: the passage of legislation to ensure teen parents the right to remain in school and the expansion of services to pregnant and parenting teenagers to enable them to do so. See Nord et al., 1992.

[10] The work of Geronimus and Korenman has made it clear that the studies that appeared in the 1970s and early 1980s exaggerated the independent effect of an early birth on the social and economic outcomes for young women. Many researchers are now using considerably more sophisticated statistical methodologies and research designs, however, and are finding that the negative effects associated with early childbearing cannot be attributed solely to selection (that is, to the fact that the women who gave birth as teenagers had high initial levels of disadvantage that would have produced the negative outcomes in any event). See, for example, Ahn, 1994; Grogger and Bronars, 1993; Hoffman, Foster, and Furstenberg, 1993. The general conclusions of these studies are the same; the preponderance of evidence indicates that, in the United States, early childbearing is often detrimental to the life chances of already disadvantaged women.

Given their lower educational attainment as well as gaps in their basic academic skills, young mothers find themselves at a distinct disadvantage in the labor market. There is extensive evidence that young mothers are less likely to work early on than young women who remain childless. Their early work histories are also marked by lower earnings, less prestigious jobs with fewer opportunities for career advancement, lower family incomes, and higher rates of poverty than those of women who give birth at a later age (Duncan and Hoffman, 1990; Grogger and Bronars, 1993; Hoffman, Foster, and Furstenberg, 1993).

While their educational deficits contribute heavily to teenage mothers' economic situation, another important consideration is that they have more children than do women who postpone their first birth (Heckman, Hotz, and Walker, 1985; Hofferth, 1987; Hoffman, Foster, and Furstenberg, 1993). This difference in family size is associated with both reduced labor force participation and increased poverty (Furstenberg, Brooks-Gunn, and Morgan, 1987). Extended welfare receipt is another concomitant of adolescent pregnancy, as will be seen in the next section.

While recent studies confirm that early parenthood has adverse effects on outcomes, these effects have often been shown to be indirect. The studies cited above generally indicate that age at first birth does not affect labor market outcomes directly; rather, early childbearing reduces educational attainment and increases family size, and it is these outcomes that negatively affect work force participation and wages. Thus, the research suggests specific avenues for program intervention; if programs can boost educational attainment and postpone subsequent pregnancies among participants, then negative labor market outcomes may be forestalled.

Children of adolescent mothers have been found to be at higher risk of developmental problems than children of older parents. Children born to young mothers are not only more likely to live in poor families headed by a single parent but also more likely to be reared by mothers who are less competent as parents. Researchers have found, for example, that teenage mothers are more likely than older mothers to have unrealistic expectations regarding children's developmental milestones, less likely to attend to their children's needs for verbal and other forms of cognitive stimulation, and more likely to be hostile and punitive toward their children (Garcia-Coll, Hoffman, and Oh, 1987; Landy et al., 1983; Levine, Garcia-Coll, and Oh, 1985; Parks and Arndt, 1990; Roosa, 1983). Given these findings, it is not surprising that researchers have also found that children of young mothers are raised in considerably less favorable home environments (Hannan and Luster, 1991; Luster and Rhoades, 1989; Polit, 1992).[11] Thus, teenage parenthood is a social problem with intergenerational dimensions.[12]

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[11] It should be noted that some of these studies have not been able to control fully for differences other than age between younger and older mothers. Negative child outcomes may well be the product both of teenage mothers' youth and immaturity and of their poverty and other adverse life circumstances.

[12] New Chance is one of the earliest large-scale demonstrations to have addressed the needs of both disadvantaged mothers and their children, but an increasing number of programs are being designed with such a two-generational focus. Some link the JOBS program for welfare recipients (to be described shortly) to high-quality child care or to the Head Start program. Others were developed apart from JOBS and are directed to low-income families that are not necessarily on welfare (Smith, 1991).

III. Young Mothers and Welfare: The Problem and the Policy Response

The policy context in which New Chance was originally developed and in which it has unfolded has been marked by continuing congressional and public concern about early childbearing and, in particular, about long-term welfare receipt. Because teenage mothers are so often poor to begin with, many of them turn to AFDC as a critical source of income support. Being unmarried increases the probability of receiving public assistance; a study conducted by the Congressional Budget Office using data from the National Longitudinal Survey of Youth (NLSY) found that 48 percent of unmarried teenage mothers received AFDC for at least one month within the first year after the birth of their first child, compared with 7 percent of married teenage mothers (U.S. Congress, 1990).

Teenage mothers represent a relatively small proportion of all mothers on welfare—7.6 percent of the average monthly caseload during fiscal year 1992 (U.S. Congress, 1994). Significantly, however, teenage mothers are likely to remain on the welfare rolls for extended periods, especially if they are high school dropouts.[13] For this reason, grants to teenage mothers and women who had their first child as teenagers account for the majority of welfare expenditures. A recent study found that teenage childbearing cost the country $2.2 billion annually in welfare and food stamp benefits, along with $3.4 billion in medical care costs, foster care expenses, and prison construction to house prisoners who had been the children of teenage mothers (Maynard, 1997).

What is commonly perceived as the intolerably high cost to society imposed by welfare, as well as concern about the consequences of teenage parenthood for children and families, has led to ongoing debate about how the welfare system should be changed to make it more consonant with public values. The Family Support Act of 1988 gave legislative expression to that concern. The act demonstrated a broad commitment to the concept that receiving welfare entails mutual obligations: a responsibility on the part of the recipient to participate in services intended to help her support herself and her children and a duty on the part of government to provide these services.[14] A major provision of the Family Support Act was the creation of the Job Opportunities and Basic Skills Training (JOBS) Program, which gave state welfare agencies increased funding and incentives for delivering education, vocational skills training, job-readiness activities, job placement, and other employment-related services, either directly or through contracts with education providers, job training centers, and other agencies. The legislation also specified that to the extent that the requisite employment-related services and child care were available, states had to require all AFDC household heads whose youngest child was age 3 or older (age 1 or older at state option) to participate in JOBS unless they were deemed exempt;[15] a recipient's failure to participate without good cause could result in a reduction of her welfare grant (a "sanction," in welfare administrative parlance).

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[13] Bane and Ellwood's (1983) pioneering analysis of data from the Panel Study of Income Dynamics (PSID) indicated that nonwhite women who went on welfare after giving birth as unmarried mothers and who were high school dropouts averaged 10 years on the welfare rolls. More recent evidence suggests that multiple welfare spells and cycling on and off the welfare rolls are common among young mothers. Using data from the NLSY, which contains monthly information on welfare status (in contrast to the PSID, which records only whether or not an individual received welfare at some point during the year), Pavetti (1992) studied 424 young women who first received AFDC benefits when they were age 20 to 23. Following them for five years after the start of their first welfare spell, she found that over this period 58 percent of all welfare spells experienced by high school graduates and 40 percent of the welfare spells experienced by high school dropouts ended with a work exit, but that 60 percent of the women who left welfare for work returned to public assistance, often within the first year after leaving it.

[14] Enhanced child support enforcement provisions contained in the Family Support Act supplied new enforcement tools so that fathers as well as mothers would support their children.

To the extent that resources were available, states also had to require that single teen parents without a high school diploma or GED attend school or other education programs, regardless of the age of their children. The purpose of this provision was to prevent teenagers from dropping out of high school as well as to induce young mothers who had already dropped out to complete their schooling. Because of financial constraints, however, it appears that few states had implemented these requirements on a large scale by the time New Chance was operating fully.[16]

For the majority of New Chance enrollees, participation in JOBS would have been considered mandatory if JOBS programs in the states or localities in which they lived had required teenage mothers to participate in education services.[17] Most New Chance sites used JOBS funding to pay for occupational skills training, support services (for example, child care or transportation), or program staff, but only a small percentage of New Chance enrollees were ever subject to a JOBS participation requirement.

The welfare reform legislation enacted by Congress in 1996 transforms the very nature of public assistance by eliminating the entitlement status of welfare that had been in place since AFDC was created. Individuals who meet the criteria for receiving assistance are no longer assured of getting it; instead, states receive block grants that they can use to provide aid to needy families, operate welfare-to-work programs, prevent and reduce the rate of out-of-wedlock pregnancies, and encourage the formation of two-parent families. The block grant structure increases the likelihood that some applicants will be denied assistance because the state has run out of money to support them. A five-year limit on the amount of time an adult can receive federally funded welfare aid is another means of reducing assistance caseloads and costs, as well as of spurring welfare recipients to take jobs and leave the welfare rolls before reaching the time limit.

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[15] Welfare recipients could be deemed exempt, for example, if they had other responsibilities that required them to stay at home, such as caring full-time for a disabled child, or if they were already working 30 hours a week.

[16] Twenty-five states responded to a 1992 survey conducted by the Center for Law and Social Policy to determine the proportion of AFDC teen parents in the state who were enrolled in JOBS. Of the 25 reporting states, 4 (Florida, North Carolina, Ohio, and Oklahoma) accounted for over half of all teen parents in JOBS. Eight states reported that over 35 percent of the teen parents on their AFDC caseloads were JOBS enrollees. Some states considered teen parents "in JOBS" if they were on the "active caseload," however, whether or not they were actively engaged in a component such as education or job training (Levin-Epstein, 1993).

[17] At the time they entered New Chance, 69.6 percent of New Chance enrollees could have been mandated to participate in JOBS if the states they lived in so required; 64.3 percent were under age 20 and did not have a high school diploma or GED, while 5.3 percent were 20 or older and had no children under age 3. An enrollee's status vis-à-vis JOBS could, however, change during the course of her participation in New Chance.

Other provisions of the new law seem to have as their principal objective the enforcement of desired behavioral norms. States are permitted to impose "family cap" rules prohibiting additional cash benefits for children born to women after they have gone on assistance. Such rules give expression to legislators' belief that welfare recipients should not continue to have children that taxpayers will have to support. A prohibition on the use of block grant funding to support an unmarried parent under age 18 who has not completed high school or a GED program (unless the parent is currently pursuing such a credential) is intended to promote high school attendance and completion (and, ultimately, to increase prospects for self-sufficiency).[18] A ban on the use of block grants to support unmarried parents under age 18 who are not residing with a parent, legal guardian, or other adult relative is intended to ensure that teenagers will receive adult supervision (and presumably behave more responsibly as a result). And a requirement that states have a certain proportion of their adult caseload (rising from 25 percent in 1997 to 50 percent in 2002) working in subsidized or unsubsidized jobs—or in work experience and community service positions, if sufficient private-sector employment is not available—is meant to foster and enforce work effort among welfare recipients.[19]

Before the federal welfare legislation was enacted, many states had sought waivers of existing federal regulations to implement some of the same reforms now incorporated in the new law. Thus, for example, as of August 1996, 38 states and the District of Columbia had requested authority to impose time limits of some kind on welfare receipt, and 21 had sought to put family caps into place.

New Chance was not implemented in an environment of time limits, family caps, residence requirements, and work obligations. Thus, this report cannot speak to what young mothers will do in the presence of these provisions. But it contains a great deal of information about what New Chance sample members, at least, have done and thereby highlights some of the complexities that need to be addressed in translating broad policy initiatives into specific programs and regulations. For example, findings on the intermittent nature of many sample members' employment are relevant to the question of how a time limit would be enforced, and under what circumstances exemptions or extensions might be granted. The substantial amount of job turnover New Chance enrollees experienced suggests that this is an issue with which community service employment programs might have to deal. And high rates of repeat childbearing raise issues about the extent to which family caps would have the desired effects in curbing family growth.

In short, while the study cannot provide definitive answers, it can focus policymakers' attention on important questions and supply information about the possible consequences of certain policy choices.

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[18] States could opt out of both the family cap and the denial of benefits to unwed teenage mothers.

[19] Under the new federal legislation, adult education, a major component of the JOBS program, plays a much reduced role; high school attendance or attendance in education programs directly related to employment is confined to those under age 20 without a high school diploma or GED.

IV. The New Chance Program Model

The 16 New Chance sites generally adhered to a specific model of service delivery. MDRC provided the sites with detailed guidelines concerning criteria for program eligibility and attributes of the treatment itself (that is, the services offered, their schedule and arrangement, and the interpersonal environment of the program). Within these parameters, the sites had some flexibility to organize activities in ways that met their own circumstances, and over time the program model continued to evolve in response to staff perceptions of participants' needs. Table 1.3 summarizes key features of the program model; a more complete discussion of the components and their implementation appears in Chapter 4.

A. The Target Population

New Chance was directed toward young women aged 16 to 22 who had given birth as teenagers and who at enrollment had neither a high school diploma nor a GED and were receiving AFDC. On the basis of calculations from the U.S. Census Bureau's Survey of Income and Program Participation, it is estimated that every month between May and August 1993 there were some 271,000 young mothers on the welfare rolls nationwide who met these criteria and who were not enrolled in school.[20]

By defining the target population in this way, program planners sought to address the needs of a group of young women who, as has been noted, were at unusually high risk of long-term poverty and welfare receipt and who, prior to the late 1980s, had typically been overlooked by welfare employment and job training programs because of their poor employment prospects and need for child care. Recognizing that some young women might be very needy but might not meet all these criteria, MDRC allowed sites to enroll up to 25 percent of applicants who were high school graduates but read below the ninth-grade level or who were economically disadvantaged but not on AFDC. Only 11 percent of program enrollees were admitted under this allowance, however.

To be eligible, young women also had to be able both to take full advantage of the program's services and then to make the planned transition to employment. For this reason, they could not be pregnant at the time of enrollment. Pregnant applicants were instructed to reapply after giving birth. Applicants were not required to submit proof that they were not pregnant, however, and a few pregnant women were admitted to the program; they may not yet have been aware of the pregnancy, or they may have concealed it from program operators.

B. The Program Structure and Services

The New Chance model adopted a holistic approach aimed not only at building participants' academic and vocational skills—that is, their "human capital"—but also at helping

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[20] An additional 80,000 women were enrolled in school either full-time or part-time. Because the Survey of Income and Program Participation did not ascertain whether or not respondents had a GED, however, the 271,000 figure probably overestimates somewhat the number of young women who would have been eligible for New Chance.

Table 1.3

The New Chance Model

Target Group

Mothers 16 to 22 years old who (1) had first given birth at age 19 or younger, (2) were receiving AFDC, (3) did not have a high school diploma or GED, and (4) were not pregnant when they entered the program

Program Structure and Services

Service Components:

Orientation

Phase I

Employment preparation components: Adult education (adult basic education–ABE–and GED preparation), career exploration, pre-employment skills training

Components to enhance personal and child development: Life Skills and Opportunities curriculum, health education and health care services, family planning, adult survival skills training, parenting education, pediatric health services

Phase II

Employment preparation components: Occupational skills training, work internships, job placement assistance

Case management

Child care

Service Emphasis: Integration and reinforcement in each component of all program messages and skills

Service Structure: Sequential phases of program activities, relatively long duration (up to 18 months), high intensity, primarily on-site service delivery

Environment: Small, personal programs; warm and supportive, but demanding, atmosphere

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them become mature, confident, and healthy adults and parents. Along with providing specific services, program staff were expected to strive to build enrollees' self-esteem and their belief in their own ability to change their lives for the better.

As Table 1.3 shows, the model called for the program treatment to begin with an orientation, sometimes lasting several days. During a participant's first several months in the program—Phase I—most services were delivered at the program site. At this stage, education was the primary New Chance activity. Typically, two to three hours of a participant's six-hour day were spent in the kind of education commonly referred to as "adult education and literacy" or simply "adult education." Such programs provide (1) adult basic education (ABE)—instruction in reading, math, and composition at or below the eighth-grade level; (2) instruction in preparing for the General Educational Development (GED) test, passage of which is intended to signify knowledge of basic high school subjects; and/or (3) classes in English as a Second Language (ESL). (ESL instruction, however, was not part of the New Chance program because virtually all the participants were native speakers of English.)

Depending on her level of academic skills at enrollment, a participant embarked on a curriculum preparing her to take the GED test or on an ABE curriculum, moving on to GED preparation when she was ready. Some sites organized separate classes for ABE and GED students; others integrated ABE and GED students in the same classroom, a feasible arrangement because most such classes emphasized individualized, self-paced instruction.

Employability development classes were devoted to such topics as possible careers and job-seeking techniques. Various personal development services were also offered during Phase I: health education classes; health care services either on-site or through linkages to local hospitals and clinics; and, at some sites, health care services; family planning instruction; Life Skills and Opportunities (LSO) classes that used a curriculum developed especially for the New Chance Demonstration and emphasized decision-making and communication skills; and "adult survival skills" (topics such as budgeting or legal rights and responsibilities that were sometimes included in other components and sometimes covered in a separate class). Finally, in keeping with the program's two-generational focus, Phase I included services to promote child development: parenting classes, pediatric health care services, and child care.

During Phase I, the New Chance schedule was much like that of a regular school. Typically, the program ran from 9 A.M. to 3 P.M. five days a week.[21] Daily attendance at all classes was expected. In other respects, however, New Chance was designed to be very different from high school. For one thing, the services mandated in the New Chance program design, while distinct, were also intended to be integrated. Components were expected to complement and reinforce one another and to present participants with a consistent set of program messages interwoven through all program activities. For example, in a parenting class students might make notes about their children's attainment of developmental milestones, thereby gaining additional practice in writing; in a life skills class, students might develop child care budgets for two versus three children in order to get a clearer picture of the financial costs of having another child.

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[21] Five sites opted for a four-day-a-week schedule, reasoning that if participants had a specific time for appointments (such as with doctors or welfare workers), they would be less likely to miss other classes. Staff at these sites typically used Fridays for meetings and preparation of lesson plans.

Even more important, local programs were intended to be small in size in order to promote an intimate and personal environment. Guidelines called for sites to enroll 100 participants over 12 to 18 months and to serve about 40 enrollees at any given time; case managers' caseloads were to include no more than 25 active participants (although they often exceeded this level in practice). Staff were expected to promote participants' development by creating an atmosphere that was supportive, with praise for both large and small accomplishments, but also demanding and marked by high expectations.

Receipt of a GED was envisioned as only the first step toward self-sufficiency. After a participant received this credential (or if she had not earned a GED but had been in the program for five months), the guidelines called for her to enter Phase II, which could include vocational training, paid or unpaid short-term work experience (called "work internships"), and job placement.[22] College, although not formally considered a Phase II component, was also a post-GED activity for a number of young women. Most of these activities took place away from the program site, although some sites offered on-site skills training. Case managers were expected to monitor participants' progress and to provide guidance and support not only while the young women were on site but also after they moved on to training, college, or jobs. Young women were permitted to remain in New Chance for 18 months and to receive child care, at no cost to themselves, throughout this period; the model also called for up to one year of additional follow-up by case managers.

V. Experiences of Other Programs for Young Mothers

To provide a backdrop for the findings presented in this report, it is useful to consider the experiences of other research and demonstration programs serving teenage mothers on welfare, focusing where possible on their records in reaching young mothers who were high school dropouts. Four such programs, key features of which are summarized in Table 1.4, are particularly relevant:

- **The Learning, Earning, and Parenting (LEAP) program.** Developed by the Ohio Department of Human Services and operated since 1989 by county human service departments, LEAP is an unusual statewide initiative that relies principally on financial incentives and penalties to promote school attendance among pregnant and parenting teenagers on welfare. The program requires teenage mothers and pregnant teens who do not have a high school diploma or GED and who are on welfare to stay in school or, if they have dropped out, to

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[22] The stipulation that participants move on to skills training or a work internship after five months was intended to keep young women focused on employment as the ultimate goal, as well as to prevent boredom and discouragement on the part of young women who were unsuccessful in earning a GED. As will be seen in Chapter 3, however, sites did not systematically adhere to this guideline.

Table 1.4

Programs Serving Adolescent Parents: Selected Dimensions

Dimension	New Chance	Learning, Earning, and Parenting (LEAP)	Teenage Parent Demonstration (TPD)	JOBSTART (Young Mother Subgroup)
Status	Demonstration	Ongoing program	Demonstration	Demonstration
Coverage	Selective within a highly targeted segment of the welfare caseload (16- to 22-year-old women who were dropouts and had given birth by age 20) in 16 sites	Broad, focusing on all teenage welfare recipients in Ohio who were custodial parents age 19 or younger without a diploma or GED certificate[a]	Broad, focusing on all new teenage welfare recipients in three sites who were custodial parents with only one child and, in one site, those pregnant with their first child	Selective within a population of 17- to 20-year-old disadvantaged teenagers who were dropouts, in 13 sites
Participation mandate	Voluntary for most participants	Mandatory: Noncompliance resulted in a sanction ($62 per month); compliance resulted in a financial bonus ($62 per month)	Mandatory: Noncompliance resulted in a sanction's being lifted only when the teenager came back into compliance	Voluntary
Level of education of youth at intake	Low: Nearly all were young mothers who had left high school without a diploma and lacked a GED	Mixed: At intake, 49.9 % of LEAP enrollees were in school; 51.1% were out of school	Mixed: At intake, 43.7% of TPD enrollees were in school, 32.7% had a diploma or GED, and 23.6% were dropouts	Low: All were young mothers who had left high school without a diploma, lacked a GED, and read below the 8th-grade level
Services/financial incentives	ABE/GED, skills training, work experience, employment preparation, career exploration/ counseling, case management, job placement, life skills instruction, family planning and health education, parenting education, personal and group counseling, pediatric and maternal health care at some sites, child care (often on-site)	High school, GED, case management, support services; parenting classes in some high schools; on-site child care and additional services in some counties; financial sanctions and bonuses based on attendance	High school, GED, job search, skills training, summer employment, case management; workshops on family planning, motivation, life skills; attendance sanctions	ABE/GED, skills training, work readiness and life skills training, counseling, assistance with child care, job placement assistance

(continued)

Table 1.4 (continued)

Dimension	New Chance	Learning, Earning, and Parenting (LEAP)	Teenage Parent Demonstration (TPD)	JOBSTART (Young Mother Subgroup)
Organizational structure	Program offered through community-based organizations, schools, and municipal organizations	Program coordinated through welfare agencies	Program coordinated through welfare agencies	Program offered through community-based organizations, schools, and Job Corps centers

SOURCES: Bloom et al., 1993; Cave and Doolittle, 1991; Maynard, Nicholson, and Rangarajan, 1993; Quint et al., 1994.

NOTE: [a]Almost all custodial parents in the LEAP study were women, but there were also a few men.

return to school or enter a program to prepare for the GED test. It offers both positive and negative financial incentives for them to do so; a bonus is added to the household's monthly welfare grant to reward good attendance, while the grant is reduced to penalize poor attendance. By improving the teenagers' school attendance in the short term, LEAP seeks to increase the likelihood that they will complete school and, in the longer term, find jobs and leave welfare. The program also offers case management and support services to students. (For a full description of LEAP, see Bloom et al., 1993; Long et al., 1996.)

- **The Teenage Parent Demonstration.** Funded by the U.S. Department of Health and Human Services, this demonstration, like the LEAP program, offers an opportunity to study the effectiveness of mandatory-participation programs for teenage parents. Operated from late 1987 to mid-1991 in Newark and Camden, New Jersey and in the southern part of Chicago, the demonstration was aimed at all teenage mothers with one child who were first-time recipients of AFDC. The young mothers fell into three almost equal groups: those who were in school at program enrollment, those who were dropouts, and those who had already graduated. The teenagers were required to participate in job search, training, or education; failure to register for the program or to comply with this requirement could result in a sanction removing the teen's portion of the AFDC grant. In addition, participants received case management, child care and transportation assistance, and workshops on parenting and other topics. (For a discussion of the program, see Maynard, Nicholson, and Rangarajan, 1993.)

- **The JOBSTART Demonstration.** This demonstration, implemented at 13 sites across the country between 1985 and 1988, sought to increase employment and earnings among economically and educationally disadvantaged youth. Enrollees, who volunteered for the program, were between 17 and 21 years old and were high school dropouts reading below the eighth-grade level. About a quarter of them were young women with children, and about 60 percent of these young mothers received AFDC on their own case. The program provided participants with education services (instruction in basic academic skills and GED preparation) and vocational training, either concurrently or sequentially. All sites provided transportation and child care assistance; the availability of other services (such as life skills instruction) varied greatly from one site to another. (See Auspos et al., 1989; Cave et al., 1993.)

- **Project Redirection**. Project Redirection was directed toward teenagers aged 17 years or younger who had neither a high school diploma nor a GED and were either receiving or were eligible to receive AFDC. Some 60 percent of the enrollees were out of school at program entry. During the main demonstration phase, between mid-1980 and the end of 1982, the program operated at four sites, where an evaluation of the program's impacts was conducted; it was subsequently expanded to an additional seven sites. The

> program's objectives were to help participants (who volunteered for the program) to return to or remain in school, delay subsequent pregnancies, and acquire employability and life management skills. The program's strategy was to link participants with existing services in the community and to support these "brokered" services by providing on-site workshops, peer group sessions, and individual counseling. It also paired teenagers with adult mentors, community women who volunteered to provide ongoing support, guidance, and friendship to the teens. (See Levy, 1983; Polit, Quint, and Riccio, 1988.)

For the most part, the evaluations of these programs were performed after planning for and implementation of New Chance were well under way. All the program evaluations except for that of Project Redirection used a random assignment design—the most rigorous possible—to measure program effects on members of the treatment group. Later chapters of this report cite specific impacts of these programs on the outcomes of interest. At the outset, however, it may be useful to summarize six broad findings of the evaluations of these programs. Together they suggest that New Chance program operators confronted major challenges, especially in working with a hard-to-serve population of young mothers who had dropped out of high school.

1. Young mothers participating in these programs have confronted many serious obstacles to advancement. All four studies indicate that young welfare mothers face many barriers to success, quite aside from poor school records and early parenthood. Significant issues among enrollees in the Teenage Parent Demonstration, for example, included high levels of depression, low self-esteem, distrust of others, dysfunctional family situations, a lack of stably employed or happily married role models, negative peer groups, and unsupportive attitudes of male partners. Among the 18 teens examined in depth in an ethnographic study of Project Redirection (Levy, 1983), at least two had been raised by abusive parents (and one was herself suspected of child abuse), a few were estranged from their mothers, two were frequently abused by their boyfriends, and one may have been involved in prostitution. Alcoholism and drug abuse were common in the homes of a significant minority of the teenagers and their parents. Strikingly, a large fraction of teenagers in LEAP saw their high schools as unruly and dangerous (Bloom et al., 1993).

2. Securing high levels of attendance has been an issue in mandatory and voluntary programs alike. In a typical month, about one-quarter of all LEAP teenage mothers (including those enrolled in school at the outset as well as dropouts) had good enough attendance to warrant a bonus, while about one-fifth were slated for a sanction because they were not enrolled in school or their attendance was poor. In the Teenage Parent Demonstration, sanctions were also frequently levied because of noncompliance both with the initial registration requirements and with ongoing participation requirements.

The fact that a program serves volunteers does not mean that enrollees can be counted on to attend regularly. JOBSTART program operators reported that a substantial proportion of the participants were frequently absent from classes, with some students routinely missing classes once or twice a week and others attending regularly for some weeks but then not showing up for a week or more at a time. In addition, 13 percent of the participant sample interrupted their participation but then returned to the program; for women, the average length of inactivity was about two months. School absenteeism was a significant problem among Project Redirection participants, with teenagers who were enrolled in GED programs attending only 50 percent of the time and those enrolled in regular or alternative high schools attending only about three-quarters of the time.

3. *Programs have had varying records in helping young mothers to secure education credentials and employment, but in most cases the majority of enrollees remained on welfare and in poverty.* In the Teenage Parent Demonstration, there were large impacts on school enrollment at two years after sample intake; impacts on receipt of education credentials and on employment were much smaller and not always statistically significant. The vast majority of both experimentals and controls were living in poverty at the time of the follow-up surveys. Young mothers in JOBSTART were considerably more likely than young mothers in the control group to have earned a GED or a high school diploma, but impacts on employment rates and earnings were seldom large enough to be statistically significant. Nor was there an impact on AFDC receipt. At the five-year point, Project Redirection achieved employment and welfare impacts (although not educational outcomes); nonetheless, over half of the treatment group members who received AFDC at baseline were also receiving it during the fifth year of follow-up.

4. *Demonstrations have not been successful in delaying repeat childbearing among young women who have already had children.* In JOBSTART, the Teenage Parent Demonstration, and Project Redirection, young mothers in the experimental group had rates of repeat pregnancies and births that were identical to or higher than those of young women in the control group. There is qualitative evidence that, for many young women, being a mother was a source of gratification and self-esteem.

5. *Programs have generally been less successful with school dropouts than with young mothers who were still in school.* At three years after enrollees' entry into the research sample, LEAP was found to have produced positive effects on completion of a high school diploma or GED and on employment for those teenagers who were enrolled in school at random assignment; for teenagers not enrolled in school at baseline, however, there was no measurable impact (Long et al., 1996). In Project Redirection, at the five-year point, women in the treatment group who were high school dropouts at program enrollment—unlike those who remained in school—were not better off than women in the comparison group in terms of employment or welfare receipt. High school dropouts in the Teenage Parent Demonstration registered significant increases in employment and decreases in welfare payments relative to their control counterparts, but the program had no effect on earnings—a pattern that suggests that those experimentals who found employment also experienced rapid job turnover.

6. *The effectiveness of programs designed for teenagers may become evident only in the long term. Young people are sometimes better able to capitalize on what they have learned in these programs after they have gained greater maturity and their lives have become more stable.* The evaluation results of Project Redirection show the importance of such follow-up. In that demonstration, outcomes for program enrollees and members of a comparison group were disappointingly similar at 24 months after entry into the research. But at the five-year point, when most study sample members were in their early twenties (approximately the age of New

Chance sample members at the 42-month follow-up), there was considerable evidence that Project Redirection had made a difference. Project Redirection enrollees were working more hours a week and had higher weekly earnings, they were less likely to be on welfare, they had created better home environments for their children, and their children showed better cognitive skills and fewer behavioral problems. Nonetheless, most young women in both research groups remained poor and on welfare.

VI. An Overview of This Report

This report covers all three components of the New Chance evaluation: information on program implementation, data on program costs, and findings on program impacts derived from the 18- and 42-month surveys. The rest of this report is divided into eight chapters. Chapter 2 discusses the research design and associated issues, while Chapter 3 examines the characteristics of the research sample as a whole at baseline and the changes in the young women over the 42-month period. Chapter 4 considers the implementation of New Chance, focusing on program activities, the extent to which experimentals participated in them, and their cost; it also presents data on the impacts of New Chance on service receipt by experimentals and controls. The five chapters that follow detail the short- and long-term impacts of New Chance on various outcomes of interest: educational status (Chapter 5); living arrangements, fertility, health, and emotional well-being (Chapter 6); employment, earnings, and welfare receipt (Chapter 7); and parenting, child care, and child development (Chapter 8). Chapter 9 discusses the key themes and issues that emerge from this report and their implications for policymakers and practitioners.

Chapter 2

Research Design, Sample Definition, Data Sources, and Analysis Plan

I. Introduction

This chapter establishes a technical and theoretical framework for the impact analyses presented in this report. It begins with a conceptual model of how New Chance was hypothesized to produce impacts; it then presents the research design, the study sample, and the data sources used for this report. The chapter concludes by discussing how the impact analysis was structured.

II. New Chance Participation and Impacts: A Conceptual Model

The comprehensiveness of the New Chance model reflects the expectation that the young mothers eligible for New Chance services faced multiple barriers to achieving self-sufficiency and personal growth. It also reflects the fact that the program sought to affect a broad range of outcomes for both the women participating in the program and for their children.

Figure 2.1 presents a conceptual model that summarizes the pathways by which the program was hypothesized to yield impacts. A few caveats should be noted. First, the model specifies (in the various boxes) only those factors that have been measured as part of the research effort and thus may be incomplete. Many factors that affect the course of a young mother's life may not have been captured adequately; nor, for the most part, could the program address them. In particular, it is recognized that the young mothers lived in complex family situations and community environments that were powerful forces in their lives. These (mostly unmeasured) contextual influences are acknowledged at the bottom of the figure.[1] Also, for simplicity's sake, the figure does not illustrate all the possible feedback loops among the various factors shown. For example, a child's characteristics affect a mother's parenting behavior, and that parenting behavior in turn affects the child's characteristics. The figure seeks primarily to identify key elements in a hypothesized causal chain.

In this figure, the various "columns" correspond to major points of data collection. Background characteristics of the mother and (to a very limited extent) the child were measured when the young mothers applied to the program. Program inputs were measured throughout the operation of New Chance. Short-term outcomes (covering the first year and a half of follow-up) were obtained during in-person interviews with mothers approximately 18 months after they

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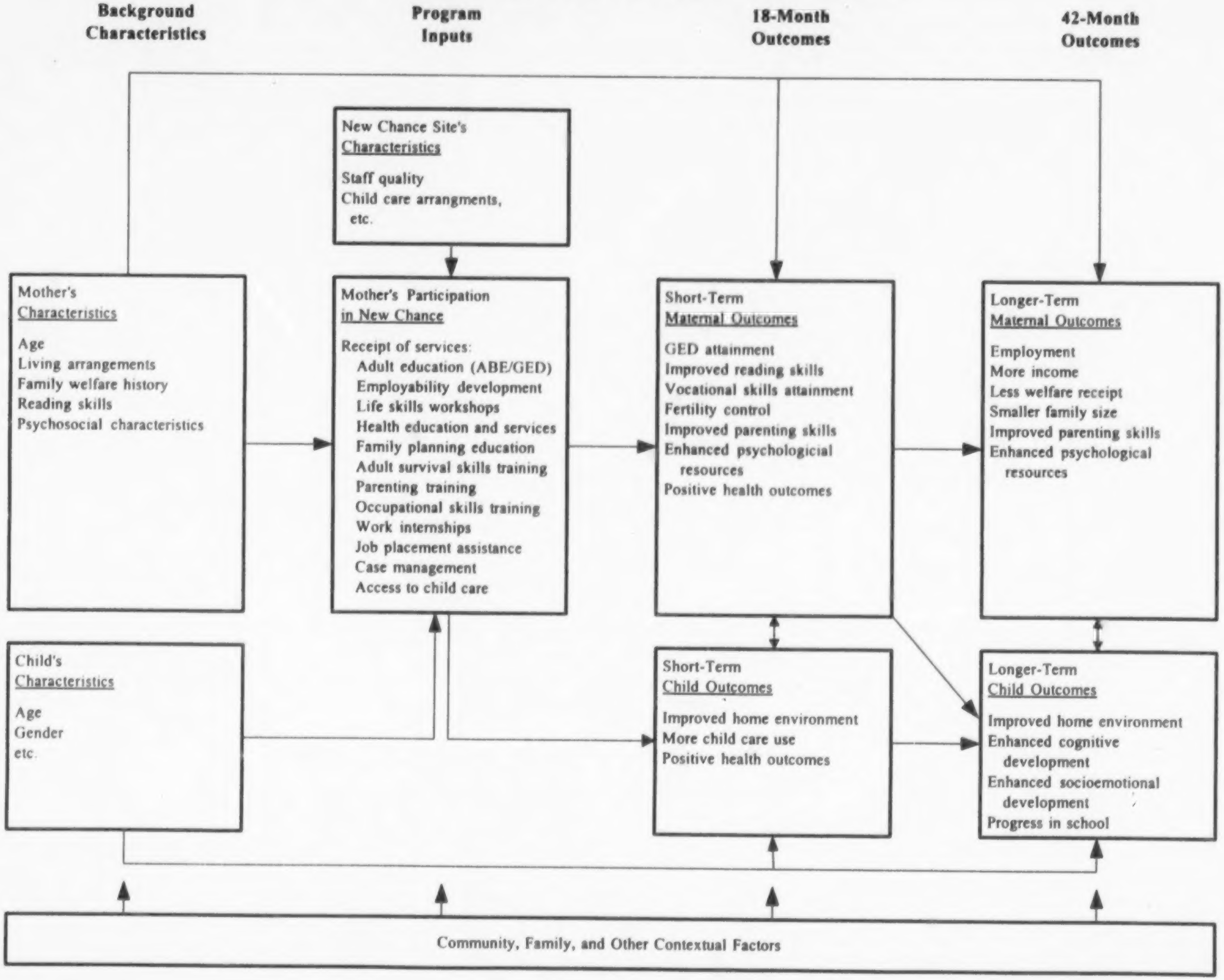
[1] Examples include family characteristics, the local labor market, neighborhood characteristics, welfare rules, and the availability of social services other than those offered through New Chance.

Figure 2.1

A Simplified Conceptual Model of the Effects of New Chance


Background Characteristics
Program Inputs
18-Month Outcomes
42-Month Outcomes
New Chance Site's Characteristics
Staff quality
Child care arrangments, etc.
Mother's Characteristics
Age
Living arrangements
Family welfare history
Reading skills
Psychosocial characteristics
Mother's Participation in New Chance
Receipt of services:
Adult education (ABE/GED)
Employability development
Life skills workshops
Health education and services
Family planning education
Adult survival skills training
Parenting training
Occupational skills training
Work internships
Job placement assistance
Case management
Access to child care
Short-Term Maternal Outcomes
GED attainment
Improved reading skills
Vocational skills attainment
Fertility control
Improved parenting skills
Enhanced psychological resources
Positive health outcomes
Longer-Term Maternal Outcomes
Employment
More income
Less welfare receipt
Smaller family size
Improved parenting skills
Enhanced psychological resources
Child's Characteristics
Age
Gender
etc.
Short-Term Child Outcomes
Improved home environment
More child care use
Positive health outcomes
Longer-Term Child Outcomes
Improved home environment
Enhanced cognitive development
Enhanced socioemotional development
Progress in school
Community, Family, and Other Contextual Factors

applied to the program, and longer-term outcomes were captured in a second follow-up interview at the end of 42 months.[2]

According to this model, the young women's background characteristics were hypothesized to influence the extent of their participation in New Chance activities. For example, young women who were depressed at baseline were expected to participate less than women who were not depressed.[3] Participation was also expected to be affected by characteristics of the site, such as staff quality, the presence or absence of on-site child care, and so on.

The young mothers' participation in New Chance, in turn, was expected to have short-term impacts on a broad range of outcomes for both the mothers and their children. The model also acknowledges that, independent of program participation, the initial characteristics of the mothers and their children would influence both short-term and longer-term outcomes. For example, women with good reading skills at the outset might be more likely than poor readers to acquire a GED and to have better employment options.

Each New Chance program component was intended to affect certain outcomes. For example, participation in adult education classes was expected to increase reading skills and educational (especially GED) attainment. Through workshops and counseling on family planning, the young women were expected to enhance their use of contraceptives and their fertility regulation behavior. Through parenting workshops, they were expected to improve their parenting skills and their ability to foster a favorable home environment for their children. Short-term impacts were, in turn, expected to affect favorably longer-term outcomes. According to the model, the gains a young woman realized while in the program—for example, increased educational and vocational skills—would, over time, result in a greater likelihood that she would be employed and a smaller likelihood that she would receive welfare.

The developmental outcomes for participants' children were expected to be positively affected by New Chance, both directly through services provided to them and indirectly through effects on their mothers. It was expected that, in the short run, the mothers' participation in New Chance would have favorable effects on the children's home environment and health. The mothers' participation was also expected to alter their children's experiences with high-quality nonmaternal child care. It was hypothesized that, in the longer run, these short-term effects—as well as the hypothesized effects on family income and the mothers' educational attainment—would improve the children's cognitive and socioemotional development and their progress in school.

In summary, the framework presented in Figure 2.1 shows that several factors working together were expected to affect the long-term outcomes for young mothers: their initial level of resources and characteristics, the quality of New Chance services, the women's level of participation in those services, and their success in achieving the program's short-term goals. Variations in these factors may help explain why New Chance produced impacts for some, but not all, people in some, but not all, sites. Efforts to analyze the separate influence of these factors are discussed in the last section of this chapter.

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[2] This second interview covered the entire 42-month follow-up period for sample members who did not answer the 18-month survey.

[3] "Baseline" refers to the point when background information on sample members was collected, just prior to their being randomly assigned to the experimental or control group. It also marks the starting point for the follow-up period. Thus, the phrases "characteristics at random assignment" and "characteristics at baseline" or "baseline characteristics" are synonymous and are used interchangeably in this report.

III. The Study Design

A. The Random Assignment Strategy

The New Chance Demonstration used random assignment, a lottery-like procedure, to divide New Chance applicants into two groups: an experimental group and a control group. While members of the experimental group were offered access to New Chance, control group members were excluded from the program, though they were free to seek services elsewhere in the community. (In order to lessen concerns about denial of services, women assigned to the control group were given a list of alternative programs and service providers.) Random assignment ensured that, aside from random variation in individual characteristics, experimentals and controls were similar in all other respects; the only systematic difference between the two research groups was their access to the New Chance program.[4] As a result, differences in program outcomes between the two groups (such as educational attainment or welfare receipt) can be attributed to New Chance. Outcomes for the control group presumably capture what would have happened to those in the program had it not been offered to them. A control group created through random assignment is a valid reference point against which the experiences of experimentals may be compared.

Entrance into the study sample occurred between August 1989 and July 1991. The last of the 42-month interviews took place in April 1995. Figure 2.2 shows the steps in building the sample. Program guidelines called on each site to recruit 150 eligible young mothers, of whom 100 were to be assigned to the experimental group and 50 to the control group. As was noted in Chapter 1, most were volunteers not facing any mandate to participate in the program. A two-to-one random assignment ratio was adopted because it was reasoned that sites would respond more favorably to random assignment and probably would be able to recruit applicants more readily if young women knew that their chance of admission to the program was better than fifty-fifty. Eligibility for the program was determined through an initial interview, during which data on the young women's characteristics at sample entry were recorded on the New Chance Enrollment Form; a reading test was also administered to program applicants.[5] As part of the application process, applicants signed

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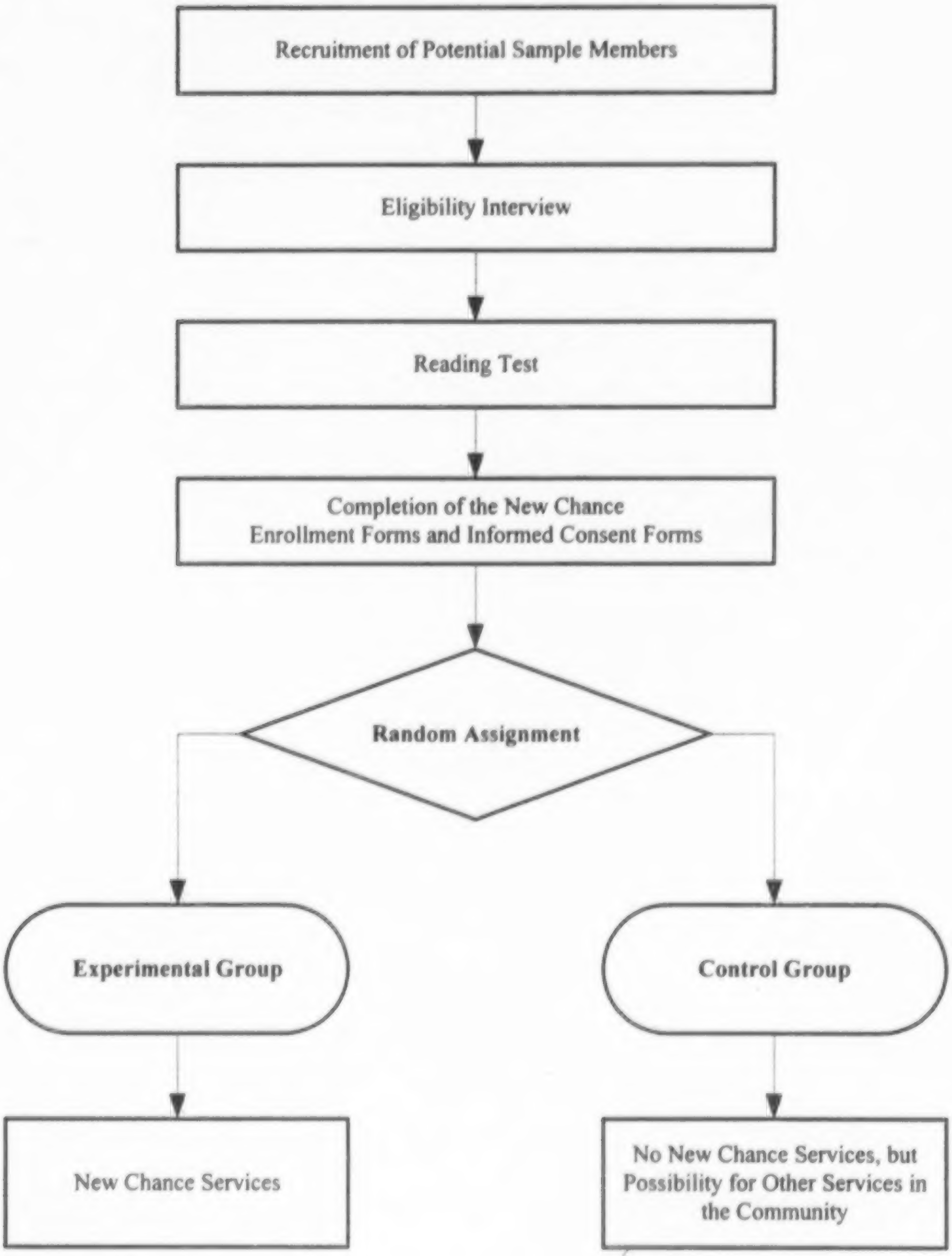
[4] The experimental design was subjected to a statistical test to ensure that the two groups created by the random assignment process were indeed similar. No systematic differences between them were found (see Appendix A for details).

[5] The test used at most sites was a short form of the reading part of the Tests of Adult Basic Education (TABE), although a few sites used the complete TABE reading test instead. For program applicants who were high school graduates, the TABE score was used to confirm that they read below the ninth-grade level, a condition of program eligibility for high school graduates, as noted in Chapter 1. For others, the TABE provided baseline information on reading skills that would be useful to the evaluation.

Figure 2.2
Steps in the Intake and Random Assignment of the New Chance Research Sample


Recruitment of Potential Sample Members
Eligibility Interview
Reading Test
Completion of the New Chance Enrollment Forms and Informed Consent Forms
Random Assignment
Experimental Group
Control Group
New Chance Services
No New Chance Services, but Possibility for Other Services in the Community

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an informed consent statement, acknowledging their participation in the study and allowing MDRC researchers to collect data on them. After a young woman's paperwork was completed, a member of the local program's staff phoned an MDRC random assignment clerk, who used a computer program to generate an assignment to the experimental or the control group.

As would be expected with a sample of volunteers, many controls found education and training services on their own, either through the list they were handed at random assignment or through their own search efforts (to be discussed in Chapters 3 and 4). Consequently, the control group in this study is not an unserved control group, a factor that affects the interpretation of experimental–control differences in outcomes. These differences, which accurately capture the New Chance program effects, do not constitute a valid measure of the effects of the services per se (versus the alternative of no services). Instead, the program effects reflect an *incremental* difference in service receipt (and, as will be seen in Chapter 4, a difference in the timing and intensity of services received). The effects of these incremental changes in service receipt cannot be generalized to the entire package of services received by the experimental group. Thus, for instance, if controls were to receive 30 hours of a certain service and experimentals received 60 hours of that service, the effect on earnings (or another outcome) of the *additional* 30 hours received by experimentals might be smaller than the effect of the *initial* 30 hours received by controls (it could also be larger). The program impact estimated by comparing the experimental and control groups (reflecting the additional hours) might not capture the full benefit of the service provided; in fact, this benefit cannot be captured with random assignment because it is not possible to randomly assign people to participate in voluntary services. Consequently, nonexperimental analyses are needed to approximate the benefit of services per se.

B. The Research Sample

In the 24 months of random assignment, 2,322 young women were assigned—1,553 to the experimental group and 769 to the control group. As is usually the case with survey data, not all of the women could be located or were willing to cooperate with the follow-up interviews. Nevertheless, the response rates were very high; 90.7 percent of all experimentals and 88.4 percent of all controls gave the 18-month interview, and 91.3 percent of the experimentals and 89.2 percent of the controls took part in the 42-month interviews.[6] In addition to sample members who were not interviewed at all, some respondents failed to answer certain questions in the survey or gave invalid answers.[7] Also, in those cases where the survey was not administered in the respondent's home, certain interviewer observations could not be made. Overall, valid data on most outcomes were available for 2,079 (98.8 percent) of the 2,105 respondents to the 42-month survey. This sample of 2,079 is the main impact sample for this report. For some outcomes presented in this report, however, sample sizes are smaller than 2,079, for several reasons:

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[6] There were 140 sample members who responded to the 42-month survey but did not respond to the 18-month survey. For these sample members, the questions on the 42-month survey were changed in such a way that they covered the entire 42-month follow-up period for many outcomes. Therefore, these "18-month nonrespondents" were included in most of the analyses presented in this report.

[7] Invalid answers include inconsistencies in the answers to related questions on the survey and out-of-range values such as "February 31."

1. Some questions were asked only in the 18-month survey. Outcomes based on these questions were unavailable for 140 sample members who answered only the 42-month survey. Therefore, the sample size for these outcomes is 1,939.

2. Most questions pertaining to child outcomes were asked only in face-to-face interviews and only when a selected child was present at the time of the interview. Therefore, data on these questions are missing when the child was not present (138 cases) or when the child was present but the sample member was interviewed by phone (91 cases).

3. Some child outcomes were collected only for children within a specific age range or for children who were attending school.

4. Some child outcomes were collected using a teacher survey (discussed in the next section). These outcomes have smaller sample sizes because they were collected only for school-age children and because their availability depended on the teachers' returning a mail-in questionnaire, which 87.1 percent did.

5. Some respondents refused to answer particular questions in the survey, often the more sensitive ones referring to sexual activity, emotional problems, and such.

Appendix B displays selected baseline characteristics for selected subsamples and for the full New Chance study sample of 2,322 young women. It also presents the results of statistical tests that assessed the statistical significance of differences in these characteristics across the samples. In brief, these tests suggested that no large and systematic differences exist between the survey respondents and the full New Chance study sample.

IV. Data Sources for This Report

This report draws on several kinds of information. Estimated program impacts (Chapters 4 through 8) were calculated from data collected with the two follow-up surveys administered to sample members at approximately 18 and 42 months after random assignment. The 18-month point was chosen for the first interview because enrollees were allowed to remain in New Chance for up to 18 months. The 18-month survey was designed to measure the short-term effects of the program, with particular emphasis on the mother's education, fertility, parenting, and use of child care. The second survey was conducted at 42 months to allow sufficient time to capture anticipated program effects on employment outcomes, welfare receipt, family income, and child outcomes.

Table 2.1 lists the various parts (or modules) of the two follow-up surveys, along with a rationale for their inclusion. The surveys were conducted in person, almost always at the sample member's home. Both surveys took on average just under 90 minutes to complete. In assessing parenting behavior, sample members' answers were supplemented by the interviewer's observations.

Table 2.1

New Chance 18- and 42-Month Follow-Up Survey Modules

Module	Rationale	18-Month Survey	42-Month Survey
Participation in education and training activities	To describe differences in the receipt of education and training services by experimentals and controls	X	X
Receipt of other services	To describe experimental-control differences in the receipt of non-education services	X	X
Educational achievement and attainment	To measure literacy outcomes and receipt of education credentials	X	X
Household structure, marriage, and residence	To describe changes in living arrangements as a possible outcome of participation in New Chance	X	X
Fertility and family planning	To measure the impact of special services targeted to changing family planning behavior and increasing the use of birth control	X	X
Psychological variables	To measure program effects on indicators of emotional well-being such as depression, self-efficacy, and access to support	X	X
Health and health care	To measure program impacts on health status, access to health care, health care utilization, and drug and alcohol use	X	X
Employment	To measure short-term employment and earnings outcomes, as well as job-seeking behavior	X	X
Income and welfare receipt	To measure program effects on the amount of family income, the combination of income sources, and receipt of public assistance	X	X
Parenting and home environment	To measure program effects on parental behavior and attitudes and on the home environment of children	X	X
Child care	To describe program effects on child care arrangements	X	X
Variables related to child's father	To describe the relationships of the children of sample members with their biological fathers or with other father figures	X	X
Child's behavioral measures	To measure program effects on behavioral problems and positive behavior		X
Child cognitive ability	To measure program effects on cognitive ability		X

SOURCE: New Chance 18- and 42-month follow-up surveys.

An important addition to the 18- and 42-month follow-up interviews was a survey of the teachers of sample members' focal children,[8] conducted after the 42-month follow-up interview. As part of that interview, sample members with a focal child who was eligible for the teacher survey (by virtue of being in school or Head Start) were asked to give the research team permission to contact the child's teacher (or Head Start instructor) and to provide contact information. Of the 1,050 respondents to the 42-month survey with an eligible child, 969 (92.3 percent) gave their permission. A teacher survey was sent to 969 teachers, of whom 844 returned the survey. This response rate of 87.1 percent is very high for a mail-in survey.

The impact analysis also relied on the Enrollment Form and baseline reading test that were completed by each sample member prior to random assignment. The form included information about the young woman's education, training, work experience, welfare history, family composition, living arrangements, and psychological well-being. Since all these items were collected before random assignment, they are fully independent of the sample member's research status (that is, membership in the experimental or control group). Thus, baseline data can be used to define subgroups of experimentals and controls for which experimental impact estimates can be generated.[9]

The source for most of data in Chapter 4 on experimentals' participation in New Chance and duration of activity in program components was the New Chance Management Information System (MIS). For each New Chance enrollee, site staff completed and sent to MDRC a monthly time sheet, which recorded the number of days she attended and the number of hours she participated in each program component. Data for the cost analysis (Chapter 4) were gathered by MDRC staff, who received detailed cost reports from the New Chance sites and conducted time studies to allocate these costs to the various program components.

Data on program operations were gathered from several sources. MDRC staff visited the 16 study sites periodically during the demonstration period and at the end of that period interviewed program coordinators to ascertain their views, which were usually based on several years' experience, on the key issues involved in running the program and on ways they would change the program model or its implementation. MDRC staff also completed reports on the site staff's experiences in operating the program components. Using widely accepted rating instruments, MDRC staff conducted assessments of the quality of child care provided by the New Chance sites (see Fink, 1994, for details). Experimentals' reactions to the program and its components were also elicited in the 18-month survey—information that is drawn on in Chapter 4.

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[8] The term "focal child" refers to a child selected for each sample member to represent the experiences of all her children. Most child-related follow-up data were collected only for this child and not for any other children in the household. For sample members who had only one child at baseline, the focal child was that child. For sample members who had more than one child at baseline, a focal child was randomly selected.

[9] Up to 51 of these baseline items were also included as covariates in impact regressions, thereby improving the precision of the impact estimates. Reported average outcomes were *regression adjusted* using one-way or two-way analysis of covariance (see Ostle, 1975, p. 461; Cave, 1987). This means that control variables were included to remove slight imbalances in baseline characteristics between the experimental and control groups as a cause of variation in the outcomes.

Finally, an important source of data for this report is a series of intensive in-person interviews that were conducted for *Lives of Promise, Lives of Pain: Young Mothers After New Chance* (Quint and Musick, 1994), a qualitative monograph on the experiences of 50 New Chance sample members who were assigned to the experimental group.

V. The Structure of the Analysis

A. Aggregate Impacts

In the chapter-by-chapter discussion of program outcomes, the effects of New Chance are first presented for the sample as a whole. These *aggregate* impacts represent the average difference between the experiences of *all* experimental group members and *all* control group members. Included in the calculations were data for experimentals who dropped out of New Chance soon after random assignment or who chose not to participate at all, as well as for those who received substantial amounts of program services. Also included were data for controls who found and received alternative services outside the New Chance program. Thus, strictly speaking, the impacts represent the incremental effects of the *additional* services New Chance provided above and beyond what control group members received on their own.[10]

B. Impacts for Subgroups and Sites

Average impacts for an entire sample often encompass a good deal of variation, some of which is attributable to differences in the characteristics of sample members and of program sites. To "get behind" the averages and find out how particular groups fared (for example, mothers who were 16 or 17 at random assignment versus those who were older, or women who had worked in the past versus women who had not), impacts were also analyzed separately for a number of such subgroups, which were defined by characteristics of the sample at random assignment (such as age and family welfare history).[11] Knowing what groups of people the program did or did not benefit could have implications for targeting and designing programs and for developing effective public policy.[12]

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[10] Most impacts were estimated using ordinary least squares (OLS), impact regressions. A Tobit estimator was used to estimate impacts on outcome variables that were truncated at the end of the follow-up period (such as "time until entered employment"). This estimator uses the observed part of the distribution of such a truncated variable to approximate the unobserved tail of this distribution (see Maddala, 1983; Tobin, 1958).

[11] Owing to smaller sample sizes, subgroup estimates are generally less reliable than estimates obtained for the full sample.

[12] As part of the subgroup analysis, the program's effectiveness was examined for different groups defined by the extent of their risk for unfavorable outcomes, as captured with the following five baseline characteristics: (1) being at high risk of depression (having a CES-D score of 24–60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

The subgroup analyses were done by creating variables that combine baseline characteristics (that is, the subgroup and site differences being studied) with a dummy variable (0 for the control group and 1 for members of the experimental group) identifying experimental status. Each one of the resulting created variables identifies the *combined* effect of being a New Chance experimental and being part of a particular subgroup for the average New Chance sample member. In other words, program effects for each subgroup are estimated holding constant all other characteristics that differentiate one subgroup from another.[13]

Similarly, knowing the results for individual sites could inform policy lessons, possible efforts to replicate New Chance, and future directions at the sites themselves.[14] For a few particularly important impacts (for example, service receipt and GED attainment), site variation is explored in some detail. In these cases, an attempt was made to isolate true site variation from variation that resulted simply because the characteristics of participants at the different sites also differed.[15]

C. Statistical Significance

The concept of statistical significance refers to the idea that a measured difference between two or more groups on a given indicator is unlikely to have arisen simply by chance. In this report, following common conventions, an impact was considered to be statistically significant if there was a smaller than 10 percent probability that it was the result of mere random variation across individuals (that is, chance).[16] In the tables, statistically significant impacts are marked with asterisks; one asterisk represents a smaller than 10 percent probability that the finding arose by chance; two asterisks, a smaller than 5 percent probability; and three asterisks, a smaller than 1 percent probability. In addition to these asterisks, many impact tables in this report have columns showing *p-values*. A p-value represents the probability that a finding is the result of random variation. For example, a p-value of .20 represents a 20 percent chance that the finding it refers to arose by statistical chance. A p-value of .10, which corresponds to a single asterisk, indicates that the chance of such a spurious finding is 10 percent. P-values allow for a more extensive assessment of the reliability of the findings to which they refer than do asterisks. For instance, an impact with a p-value of .12 would not be flagged with an asterisk but may still be considered "marginally significant," since it comes close to .10, which would have been considered statistically significant.

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[13] Statistical tests are performed to establish whether or not apparent subgroup and site differences are systematic or the result of random variation. These tests are based on the assumption that allowing the program effect to vary by subgroup or site leads to a better "fit" of the impact regression. If this improvement in fit is statistically significant, the subgroup variation is considered significant as well. In estimating subgroup impacts, the single experimental dummy is replaced by a set of interacted program variables in the impact regression. A joint F-test comparing the interacted and uninteracted regression results is used to establish the significance of the gain in the regression's explanatory power caused by this substitution, providing a measure of the statistical significance of the impact variation across subgroups (see Kennedy, 1992, p. 57).

[14] The smallest site enrolled only 69 sample members (experimentals and controls combined), and the largest, 171. Small sample sizes reduce the likelihood that even fairly large percentage differences will be statistically significant and, therefore, that the results can be ascribed to anything except chance.

[15] This was done by including all 36 non-site interactions of individual baseline characteristics with the experimental dummy in the regression equation containing the interacted site dummies. These added interactions removed any site-specific impact variation that was due to differences in individual sample characteristics, rather than being attributable to site-specific factors.

[16] Statistical tests conducted to assess the significance of regression coefficients were "two-tailed." That is, they tested whether these coefficients were significantly different from zero in either direction, rather than merely testing whether they were significantly different from zero in the *intended* direction. A significance level of 10 percent in a two-tailed test corresponds to a more conservative significance level of 5 percent in a one-tailed test.

In comparing and interpreting program effects presented in this report, it is important not to confuse *statistical* significance with *practical* significance. The size of the New Chance sample was chosen to provide enough statistical power to find meaningful effects even on outcomes with a great deal of natural variation. As a result, program effects on outcomes that are relatively stable may be statistically significant, while being too small to be meaningful in practical terms.

D. Nonexperimental Extensions of the Analysis

Most of the impact information presented in this report is based on experimental comparisons (that is, differences in outcomes between experimentals and controls, which we refer to as "the impacts"). In addition, however, many nonexperimental comparisons were made to clarify or disaggregate experimental results. Thus, for instance, nonexperimental analyses were used to examine the relationship between the level of participation in adult education (ABE/GED) and rates of GED receipt. Other examples of nonexperimental analyses featured in this report include a study of the effects of child care on child outcomes, a breakdown of earnings by living arrangement, and analyses that try to identify determinants of depression.

Nonexperimental analyses like these involve comparisons of subgroups of experimentals and controls created using sample members' *post-baseline* experiences. Consequently, the results are subject to potential selection bias.[17] For instance, sample members who received a lot of services might have been systematically different from sample members who did not. Unless removed by statistical procedures, these differences may cause bias in estimates that examine the effects of post-baseline experiences. For several key analytical questions (such as the relationship between program intensity and educational attainment), statistical techniques were used to minimize the threat of selection bias. Specifically, an attempt was made to remove selection bias from nonexperimental comparisons by using *instrumental variables*.[18] In other cases nonexperimental comparisons take the form of simple descriptive tables or regression analyses, whose interpretation remains subject to selection bias. In those cases, this fact will be clearly indicated in the discussion accompanying these analyses.

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[17] The term *selection bias* refers to the notion that a selection process usually accompanies events and decisions in a person's life, such as participation in education or training, receipt of an educational credential, or a decision to give birth. This selection process, be it an individual decision or an institutional procedure, tends to create systematic differences between those who receive services and those who do not, and these differences interfere with the assessment of the effect of the services per se. Generally, these pre-existing differences are not limited to measurable characteristics but include difficult-to-measure concepts such as motivation and aptitude, which may not be captured by measured baseline characteristics.

[18] This technique—also referred to as *two-stage least squares*—includes data for experimentals and controls and uses the experimental assignment dummy to remove selection bias from the service receipt variables, in the following procedure. Along with measured baseline characteristics, the experimental dummy is used in a first-stage regression to predict service receipt. Instead of actual service receipt, predicted values of service receipt are saved and used in a second stage to estimate effects on a criterion variable, such as GED attainment or fertility behavior. Because predicted values of service receipt are uncorrelated with the unexplained part of the criterion variable, selection bias is removed

(...continued)

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In addition to instrumental variables, other nonexperimental analyses were conducted using changes in outcomes over time as a source of inference about relationships between participation variables, educational attainment, and employment outcomes. These analyses are presented in Chapter 7, while the technical details surrounding them are discussed in Appendix C.

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from the regression of the criterion variable on service receipt. The fact that the experimental dummy is highly correlated with the receipt of service but uncorrelated with the error term in the first stage improves the efficiency of this procedure. Such a "clean" predictor of service receipt is referred to as the *instrument* in the instrumental variables technique. Unfortunately, results from this procedure are generally imprecise. They are also based on the arguable assumption that *all* program impacts on educational attainment are achieved via education services, all program impacts on pregnancy are achieved via family planning services, and so on.

Chapter 3

Sample Characteristics and Outcomes for the Control Group

I. Introduction

This chapter describes the characteristics of the New Chance sample at baseline and the experiences of sample members in the control group during the 42-month follow-up period. This description forms the primary context for the New Chance program effects presented in Chapters 4 through 8 of this report. The baseline characteristics introduce the personal history and circumstances of the young women as they entered the program, while 42 months of outcomes for the control group represent what *would have happened* to those in the experimental group in the absence of New Chance.

In this chapter, it will become apparent that New Chance sample members were highly disadvantaged as they entered the study. Following the control group over time, however, reveals that many positive changes occurred in the lives of these young women, even after they were denied access to the New Chance program. In spite of their disadvantages, there was much growth, resilience, and positive development.

Upon their entry into the study, very few sample members had graduated from high school or received a GED certificate. Few had work experience, and virtually all were receiving welfare. Many had been the victims of physical, emotional, and sometimes sexual abuse (Quint et al., 1991). Despite all these impediments to their personal development, many members of the control group managed to receive an educational credential by the end of the follow-up period. Many were working, and some had left welfare. Other outcomes also showed favorable trends over the course of the follow-up period. Family income, while still very low, increased by close to 50 percent between 18 and 42 months, as did individual earnings, while feelings of depression were significantly less pervasive at the 42-month follow-up interview than they had been at baseline.

After the discussion of sample members' characteristics at baseline, the structure of this chapter closely mirrors the structure of the report as a whole, with subsections that cover the subject areas featured in the impact chapters (Chapters 4 through 8). The first section describes control group members' participation in education and training and their subsequent receipt of educational credentials. This description is followed by a discussion of living arrangements, fertility, health, and emotional well-being. The next section describes employment outcomes, welfare receipt, and family income, and a final section is devoted to child outcomes.

II. Characteristics of the New Chance Sample at Baseline

Table 3.1 provides detailed background information on the 2,079 survey respondents—experimentals and controls—who constituted the sample for most of this report. The data were collected with the Enrollment Form completed when sample members applied for entry into the program.

The table shows that most of the young women in the sample could be considered truly disadvantaged at the time of their application to the program. The findings are consistent with the effort to target welfare recipients and high school dropouts; 94.9 percent of all sample members were receiving AFDC at baseline, and almost 94 percent had not graduated from high school or earned a GED. More than half the sample members (63.3 percent) had not worked at all during the 12 months prior to applying for New Chance. Of those who had worked, the majority (69.5 percent) earned $1,000 or less during this period (not shown in the table).

Sample members' average age at the time of application was only 18.8 years. The youngest sample members were 16 years old, while the oldest were 22. Most young mothers in the sample (65.0 percent) had one child, and for the majority of sample members (53.3 percent), the youngest child was less than a year old. Thirty percent of all sample members reported not practicing contraception the last time they had had intercourse and were thus at risk for an additional pregnancy.

The large majority (90.2 percent) of the sample had never been married before their application to New Chance. Of those who had been married, only three in ten (3.0 percent of the entire sample) were still living with their spouses at baseline (not shown in the table). A total of 11.0 percent of the sample reported living with a husband or partner at the time of random assignment.

About a third (34.3 percent) of all sample members were living with their mothers, and 32.4 percent were living in a household of their own with no other adult present. Because of the significant policy interest in requiring young mothers on welfare to live with their mothers or with other adults, the baseline characteristics of both young women who lived with their mothers and those who had other living arrangements were examined. As expected, the two groups differed in many respects.[1]

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[1] Those not living with their mothers were older and more likely to be white. More of them had been married, and they had had more pregnancies; a higher proportion had two or more children. Those not living with their mothers were less likely to have lived with both parents at age 14 and more likely to have held a job. They were more likely to be receiving AFDC on their own case and less likely to be receiving AFDC on someone else's case; they were also more likely to be receiving food stamps, to be on Medicaid, and to live in public housing. Young mothers who did not live with their mothers expected to have fewer additional children and were more likely to be using birth control. Young women living apart from their mothers were also less likely to have used child care, less likely to have a phone, and more likely to have a driver's license. There were no significant differences at baseline between those living with their mothers and those living in other arrangements with regard to the average highest school grade completed, reading level, educational aspirations, family receipt of welfare during childhood, depression, and age at first giving birth.

Table 3.1

Selected Characteristics of the New Chance Sample at Random Assignment

Characteristic and Subgroup at Random Assignment	Average or Percent
Demographic characteristics	
Age (years) (%)	
16	1.9
17	17.4
18	22.2
19	25.8
20	19.6
21	11.0
22	2.0
Average age (years)	18.8
Ethnicity (%)	
Black, non-Hispanic	52.4
Hispanic	22.8
White	22.5
Other	2.3
Marital status (%)	
Never married	90.2
Other	9.8
Number of children (%)	
1	65.0
2	26.7
3 or more	8.2
Average number of children	1.5
Age of youngest child (years) (%)	
Less than 1	53.3
1	27.4
2	11.5
3 or older	7.8
Average age of youngest child (years)	1.2
Age at first child's birth (years) (%)	
14 or under	5.2
15	12.1
16	23.2
17	26.3
18	21.2
19	12.0
Average age at first child's birth (years)	16.8
Living arrangement	
Living with (%)	
Mother	34.3
Father	8.3
Spouse or partner	11.0
No other adult	32.4

(continued)

Table 3.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Lived in a female-headed household at age 14 (%)	48.3
Lived with both parents at age 14 (%)	23.0
Education characteristics	
Highest grade completed (%)	
7th or below	2.7
8th	10.7
9th	23.0
10th	30.7
11th	27.6
12th	5.3
Average highest grade completed	9.9
Had high school diploma or GED (%)	6.1
Left school before first pregnancy (%)	37.6
Average number of years since last attended school	2.4
Reading level[a] (grade equivalent) (%)	
4th grade or below	8.8
5th grade	5.6
6th grade	9.1
7th grade	10.7
8th grade	14.5
9th grade	21.2
10th grade or above	30.0
Average reading level[a] (grade equivalent)	7.6
Desired educational attainment for self (%)	
High school diploma or GED	33.3
1-3 years of college (A.A. degree)	31.0
4 years of college (B.A. degree)	21.4
Graduate degree	10.9
Other	3.4
Desired educational attainment for child[b] (%)	
Elementary school	0.2
High school	21.7
College/post-secondary	57.0
Graduate school	21.1
Mother has high school diploma or GED (%)	52.3
Mother attended college (%)	25.5
Father has high school diploma or GED (%)	42.8
Father attended college (%)	16.2
Both parents have high school diplomas or GEDs (%)	28.9
Both parents attended college (%)	7.7

(continued)

Table 3.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Employment and welfare receipt	
Number of jobs ever held (%)	
0	21.2
1-2	33.4
3 or more	45.5
Average number of jobs held	2.6
Employed at random assignment (%)	3.0
Number of months employed in prior 12 months (%)	
0	63.3
3 or less	18.8
4-6	10.1
7-12	7.8
Prior-year earnings (%)	
\$0-\$500	80.2
\$501 or more	19.8
Length of longest job (%)	
Never employed	20.6
Less than 1 month	4.0
1-3 months	23.0
4-6 months	22.8
7-12 months	17.8
Over 1 year	11.8
Mother employed (%)	
Yes	49.2
No	42.5
Don't know	4.2
Deceased	4.2
Father employed (%)	
Yes	45.4
No	20.1
Don't know	24.8
Deceased	9.7
Receives AFDC (%)	
Own grant	87.5
Other person's grant	7.4
Not receiving AFDC	5.1
Receives (%)	
Medicaid	87.7
Food stamps	83.8
Public housing	23.6
Income from a job	3.2
Family received AFDC when sample member was growing up (%)	
Never	36.4
2 years or less[c]	18.4
More than 2 years, but not always[c]	28.7
Always	16.6

(continued)

Table 3.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Fertility-related characteristics	
Number of pregnancies (%)	
1	43.1
2	32.1
3	16.1
4	6.2
5 or more	2.5
Average number of pregnancies	1.9
Ever had an abortion (%)	23.4
When next child is expected (%)	
Not expecting another child	64.3
Within 2 years	1.7
In 2-4 years	16.8
In 5 years or more	17.2
Average number of years until next child is expected[d]	4.4
Current birth control use (%)	
Yes, using birth control	61.4
No, not using birth control	12.4
No partner/not having sex	26.2
Used birth control at last intercourse (%)	70.2
Relations with child's father	
Speaks with child's father[b] (%)	67.3
Has child support order[b] (%)	27.7
Prior and current service receipt	
Ever in occupational skills training (%)	22.6
Services received in the 60 days before random assignment (%)	
Health care for child	84.8
Family planning	23.5
Mental health	2.8
Health care for self	58.9
Parenting	11.0
Life skills	3.1
Counseling	4.0
Other services	10.6
No services	8.3
Has regular child care[e] (%)	44.6
Psychosocial characteristics	
CES-D (depression) Scale[f] score (%)	
0-15 (not at risk)	46.6
16-23 (at some risk)	25.3
24-60 (at high risk)	28.1
Average CES-D score[f]	18.1

(continued)

Table 3.1 (continued)

Characteristic and Subgroup at Random Assignment	Average or Percent
Average number of sources of emotional support	2.8
Average level of satisfaction with emotional support[g]	4.2
Average self-esteem score[h]	38.4
Average Locus of Control score[i]	22.0
Other	
Has home telephone (%)	83.6
Has driver's license (%)	27.5
Sample size	2,079

SOURCE: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[b] When a sample member had more than one child, her response refers to her first child.

[c] The family's AFDC receipt may not have been continuous.

[d] Includes only those sample members who expected to have more children.

[e] Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

[f] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[g] Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

[h] The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

[i] The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

As part of the enrollment process, the young women who applied for New Chance were also asked questions regarding their psychological status and the degree of social support they experienced.[2] Depression was measured with the Center for Epidemiological Studies Depression (CES-D) Scale. The table shows that 53.4 percent of all sample members registered scores of 16 or higher, generally considered to place them at risk for a clinical diagnosis of depression; 28.1 percent had scores of 24 or higher, indicative of high risk of such a diagnosis. Scores on scales of self-esteem and social support do not appear to be particularly low, however, and the young women in the sample recorded a higher-than-expected average score on a scale measuring sense of control over one's life. This measure may have captured their positive expectations about entering this program.

By design, the New Chance sample was homogeneous with regard to many key characteristics; all sample members were recruited from a group of disadvantaged young mothers with similar childbearing and education histories. There was still considerable variation in the sample, however. For example, at program entry 14.4 percent read below the sixth-grade level, but almost a third (30.0 percent) read at the tenth-grade level or higher. And while 16.6 percent of the young women came from families that had always received welfare when they were growing up, 36.4 percent came from families that had never received AFDC.

Variation in sample members' characteristics was especially pronounced across the New Chance sites.[3] Site-specific entry criteria were one source of such variation. For example, the Harlem and Minneapolis sites required that all program applicants read at the sixth-grade level or higher. This requirement helps to account for the fact that the average reading level at baseline in Minneapolis was the highest in the demonstration. Other differences are explained by underlying differences in the target population. For example, sites such as the Bronx, Chula Vista, Denver, and San Jose had relatively large numbers of Hispanic enrollees, while the majority of sample members in Portland and Salem were white. Other site differences are less readily explicable—for instance, the fact that only in Lexington did the majority of sample members have more than one child.

Despite this variation, sample members' baseline characteristics show that New Chance reached the intended target population of disadvantaged young mothers. At baseline, the majority of the sample members had no education credentials, very little work experience, and limited reading skills. Most young women in the sample were taking care of very young children, so child care and other ancillary services were needed to facilitate their participation in education and training activities. The high level of depression also suggests that the program reached young women experiencing emotional problems as well as practical ones.

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[2] For a discussion of the measures and scales that were used, see Quint, Fink, and Rowser, 1991, pp. 90–91.

[3] For an extensive table with sample characteristics by site, see Appendix Table E.1 in Quint et al., 1994, pp. 292–98.

III. Control Group Outcomes

A. Education, Training, and Receipt of Education Credentials

As is discussed in greater detail in the next chapter, both experimental and control group members participated in education and training activities during the follow-up period. This section focuses on participation in education activities by members of the control group, thus representing what participation levels for young women in New Chance would have been in the absence of the program. Figure 3.1 shows cumulative rates of participation in adult education (ABE/GED), skills training, and college for the control group. It appears that by the end of the follow-up period, almost 70 percent of all controls had participated in some type of adult education. Participation in skills training and college was not so widespread (38.1 and 19.9 percent, respectively), but Figure 3.1 shows that cumulative participation rates in these services continued to increase even in the last year of follow-up. In other words, it appears that even without New Chance many young women in the sample participated in education and training activities, even three-and-a-half years after they first applied to the program.

Figure 3.2 shows the total number of weeks in adult education (ABE/GED), college, and skills training as it accumulated over time for the average member of the control group. The graph shows that the average control group member had received a total of 18.9 weeks of adult education, skills training, and/or college at the time of the 18-month follow-up interview, which is when the New Chance program was expected to end for those assigned to the experimental group. Participation in education and training continued right until the end of the 42-month follow-up period, at which time the average member of the control group had received 36.3 weeks of education and training. In other words, even without access to the program the average control group member was enrolled in education or training for one out of every five weeks in the follow-up period.

Figure 3.3 shows that much of this participation in education and training was followed by the receipt of education credentials. At baseline, 93.9 percent of all sample members reported not having an education credential. For controls, this proportion shrank to 47.1 percent during the 42-month follow-up period; thus nearly half of all controls received at least one educational credential (including training certificates) during the follow-up period. More specifically, by the end of the 42-month follow-up period, 11.1 percent of all controls had a high school diploma, 32.5 percent a GED certificate, and 24.6 percent a training certificate or trade license. Again, receipt of these credentials continued beyond the 18-month period during which participation in New Chance was supposed to be completed; one out of every three controls who received her first educational credential during the follow-up period did so after Month 18.

How do these figures compare with those found for other programs serving young single mothers on welfare? Long et al. (1996, p. 73) studied teenage parents who were part of the evaluation of Ohio's *Learning, Earning, and Parenting* (LEAP) program and found that 15.0 percent of teenage parents who were not enrolled in school when they entered the study had participated in vocational training by the end of the third year of follow-up. Only 5.2 percent of

Figure 3.1
Cumulative Rates of Participation in Adult Education, College, and Skills Training by New Chance Control Group Members Within 42 Months After Random Assignment

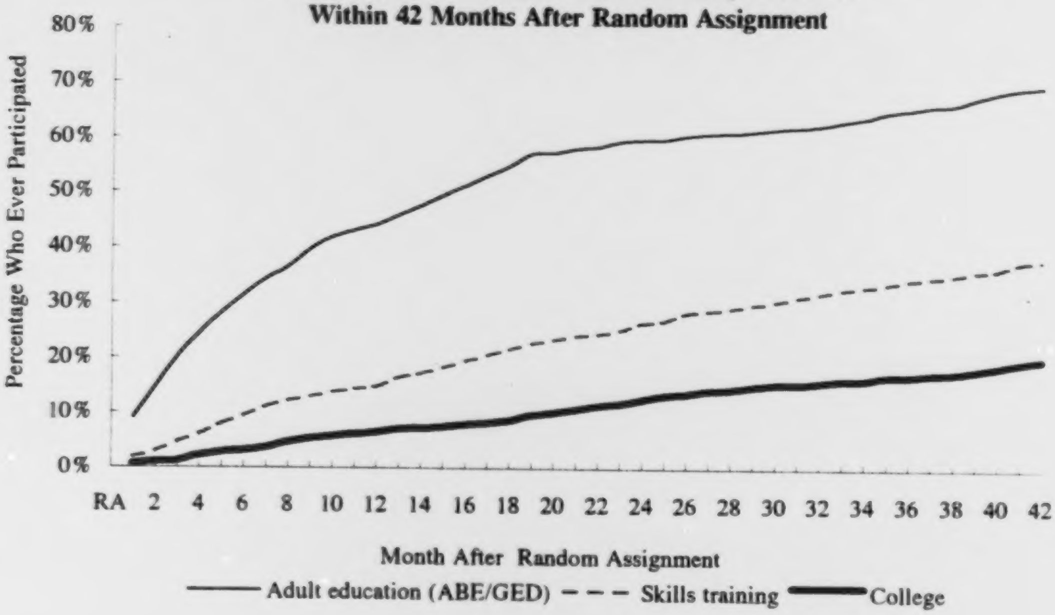
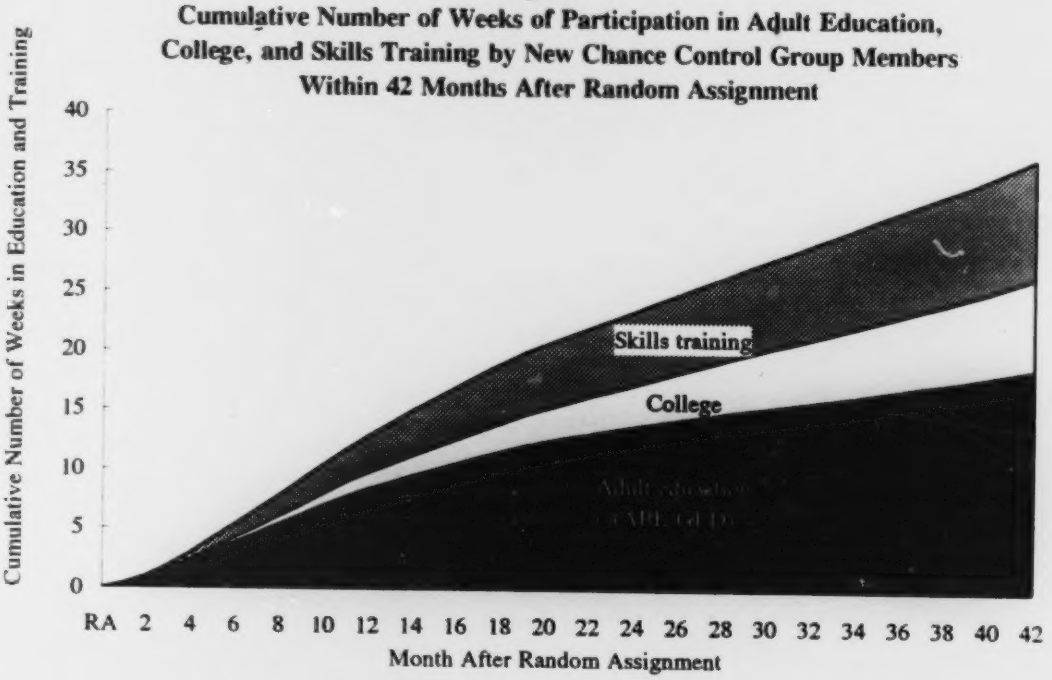


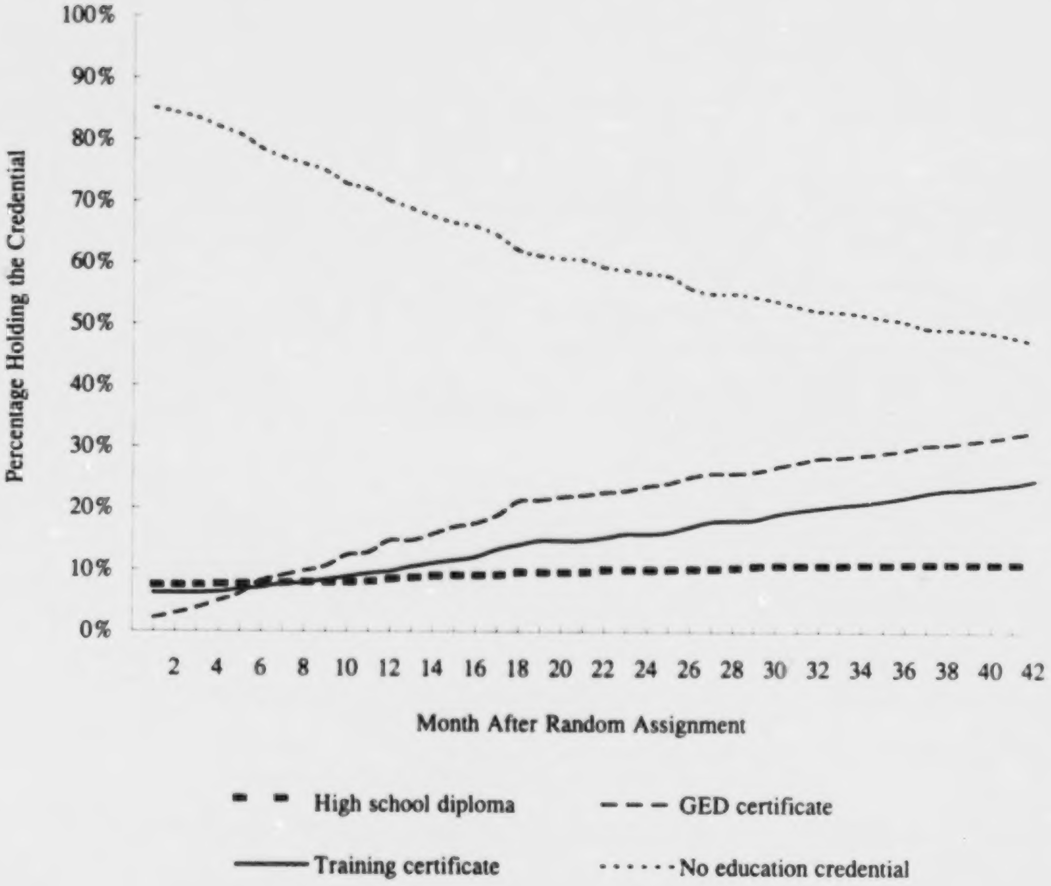
Figure 3.2
Cumulative Number of Weeks of Participation in Adult Education, College, and Skills Training by New Chance Control Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.1 for data corresponding to figures.

Figure 3.3

Receipt of Education Credentials by New Chance Control Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.2 for data corresponding to figure.

this sample had been in college by that time.[4] Comparable rates of participation for New Chance *controls* were 34.2 and 16.8 for skills training and college respectively (by the end of Month 36 of follow-up). The fact that New Chance was a voluntary program (and therefore presumably attracted fairly mature young mothers) whereas LEAP was mandatory may explain part of these substantial differences. In any case, this comparison underscores the fact that volunteers for the New Chance program were active participants in education and training activities even if they were excluded from New Chance and had to find alternative service providers.

Comparing the experiences of New Chance controls with those of their counterparts in the JOBSTART evaluation is more appropriate, as JOBSTART was also a voluntary program. This comparison shows that the young mothers in the New Chance control group received much more education and training than their counterparts in JOBSTART. Specifically, Cave and Doolittle (1991) found that during the first 24 months of follow-up, 47.9 percent of young mothers in the JOBSTART control group participated in education or skills training, for an average of 256 hours. For New Chance controls in their first 24 months of follow-up, the comparable figures were a participation rate of 72.9 percent and an average of 451 scheduled hours of education and training.

The young women in the New Chance sample were also much more likely to hold educational credentials and training certificates than their counterparts in several other evaluations of education and training programs. While a total of 43.6 percent of all young women in the New Chance control group had a high school diploma or GED certificate by the end of the follow-up, only about a quarter of the controls in the JOBSTART subsample of young mothers did (Cave et al., 1993, p. 110). On the other hand, young mothers in the JOBSTART control group were just as likely to have received a training certificate by the end of follow-up; 21.3 percent had received certificates, compared with 24.6 percent of the New Chance controls.[5]

B. Living Arrangements

Many of the young women in the New Chance sample made changes in their living arrangements during the 42 months after random assignment. This mobility is consistent with the fact that they entered the study when most of them were in transition from adolescence to young adulthood.

At the 42-month interview, most controls were found to have moved many times since their children were born. Only 5.2 percent had not moved at all, and 27.6 percent had moved five or more times. A substantial minority of the women in the sample moved away from and later returned to live with their parents. By the end of the follow-up period, however, only 20.4 percent of the women in the control group were still living with a parent or grandparent, a substantially smaller share than the 46 percent of all U.S. females aged 18 to 24 who lived with a parent.[6] The most common living arrangement was living alone with children with no one else in the household (38.9 percent).

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[4] Long et al. (1996) did not report rates of participation in high school or adult education for this group. In an earlier report, however, Bloom et al. (1993, p. 132) reported that 40.0 percent of the out-of-school LEAP sample members were enrolled in high school or adult education during the first 12 months of the LEAP evaluation. This compares with 67.2 percent for the New Chance sample in the same 12 months of follow-up.

[5] Given the different foci of the LEAP and New Chance programs, a comparison of educational credential receipt between these two programs is not entirely appropriate. A comparison of credential receipt between the out-of-school LEAP sample and their counterparts in New Chance, however, reflects positively on the latter group. Only 20 percent of out-of-school LEAP teens (experimentals and controls combined) had received a high school diploma or GED by the end of three years of follow-up, as opposed to 40.4 percent of New Chance controls.

The percentage of women in the control group living with a male partner nearly tripled over the 42 months after baseline (from 11.1 percent to 31.7 percent), and there was a fourfold increase in the percentage who were married (from 3.1 percent to 12.3 percent), although about three out of four women in the sample had never been married. Figure 3.4 shows the distribution of living arrangements reported in the 42-month interview.

C. Fertility

The majority of the young mothers in the control group (73.0 percent) became pregnant again after baseline,[7] and more than half (55.6 percent) had another baby by the time of the 42-month follow-up. Figure 3.5 shows cumulative rates of pregnancy and birth as they occurred during the follow-up period.

Most post-baseline pregnancies were described as unwanted or mistimed, but 25.5 percent of the controls said they had had at least one pregnancy that was wanted or planned. At the time of the 42-month follow-up, the majority of young mothers had two or more children; about one-third had three or more. The average age of the mother's youngest child was 2.8 at the time of the 42-month interview.

Despite the fact that at all three interviews approximately half the mothers in the control group reported not wanting any more children, one in four women in the control group failed to practice contraception regularly. Over the 42-month study period, however, the percentage of women using a prescription or surgical method of birth control rose steadily. By the end of the study, over half the control group reported using such methods.[8] About one in five women said they had been sexually abstinent for the two months prior to the final interview.

When fertility outcomes for the New Chance controls are compared with national statistics, it appears that the young women who applied to New Chance were more likely to give birth than their counterparts nationwide. In 1992, 11.5 percent of all women aged 20 to 24 gave birth (U.S. Bureau of the Census, 1995), as against 18.9 percent for New Chance controls in the last year of follow-up. On the other hand, in a comparison with similar samples from other evaluations, the rates of subsequent pregnancy and birth in New Chance do not appear to be atypical. For example,

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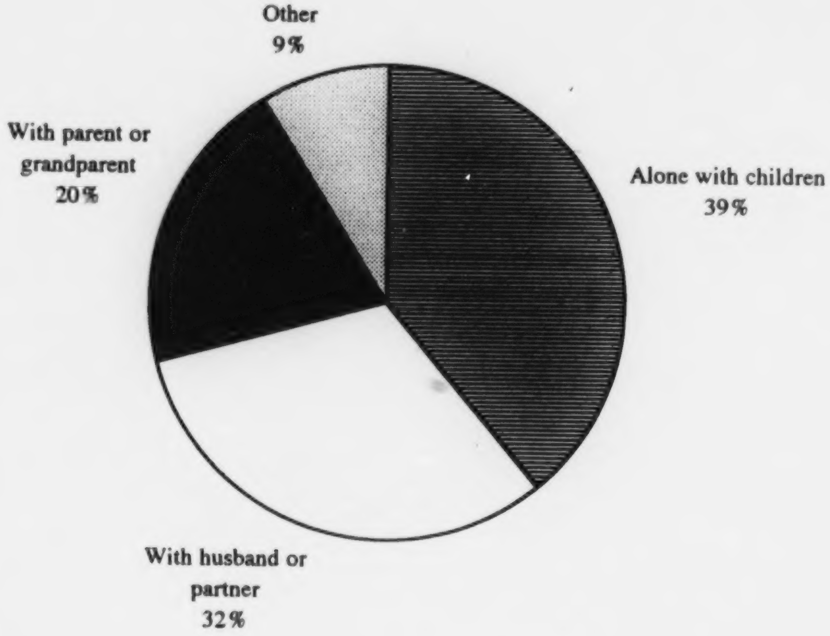
[6] U.S. Bureau of the Census, 1995, p. 56. Note that this figure includes young women who were not mothers.

[7] Virtually no sample members were pregnant at baseline; being pregnant would have disqualified them for participation in New Chance.

[8] Data from the U.S. National Center for Health Statistics (U.S. Bureau of the Census, 1995, p. 82) for 1990 reported that 27.4 percent of women aged 15–24 used a prescription or surgical method of birth control. Among sexually active women in this age group, 40.8 percent used these methods of contraception.

Figure 3.4

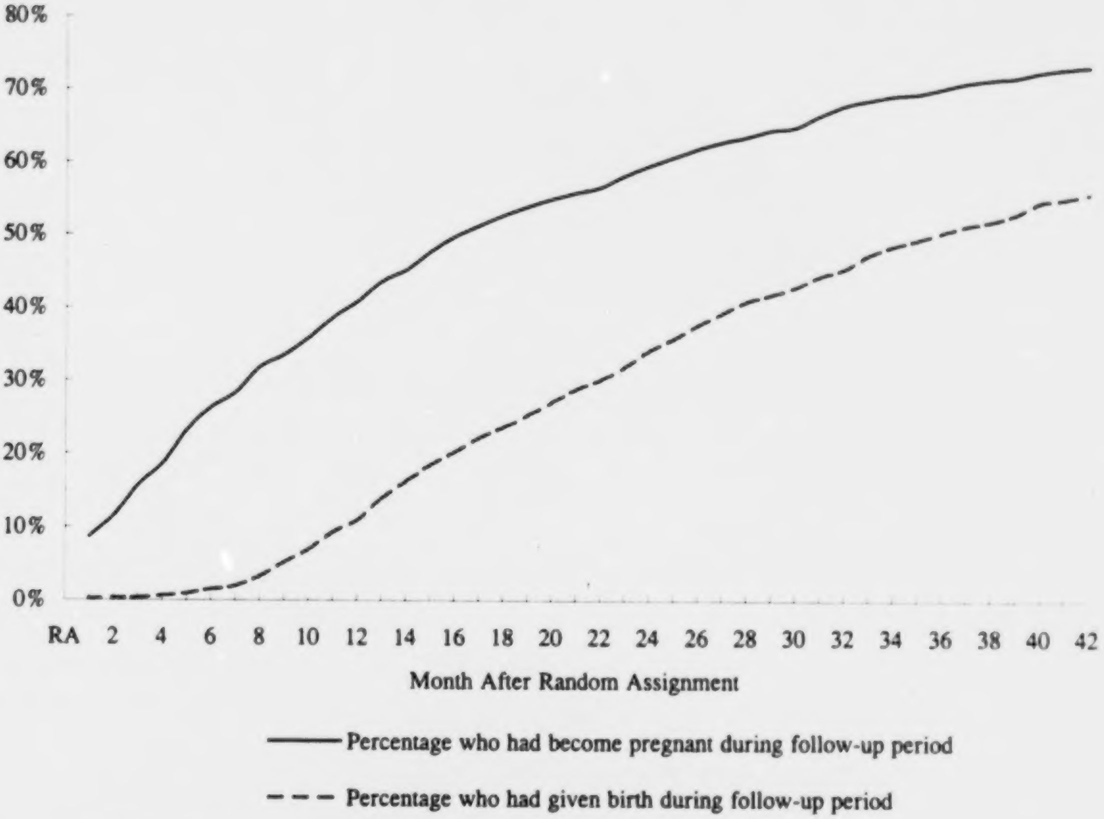
Living Arrangements of New Chance Control Group Members at 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.

Figure 3.5

Cumulative Rates of Pregnancy and Birth for New Chance Control Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.3 for data corresponding to figure.

Cave and Doolittle (1991, p. 160) reported that 53.1 percent of young women in the JOBSTART control group who were living with their own children when they entered the study became pregnant again during the first 24 months of follow-up, while 59.4 percent of New Chance controls became pregnant again during the same two-year follow-up period. Maynard, Nicholson, and Rangarajan (1993) found that 55 percent of the young women studied in the Teen Parent Demonstration became pregnant again within two years of their entry into the study (these fertility data were available for an aggregate sample only[9]). Finally, Polit, Kahn, and Stevens (1985) reported a 24-month pregnancy rate of 57 percent in Project Redirection for teens who were out of school at baseline.[10]

D. Health and Emotional Well-being

Despite the young age of the mothers (averaging 22.4 at the final follow-up), health problems were not uncommon in this sample. Only about half the mothers in the control group described their overall health as very good or excellent at the 18-month and 42-month interviews. A substantial minority (over 20 percent) had been hospitalized at some point since random assignment for reasons other than childbirth. The great majority of women had health care coverage, but the percentage without health insurance increased somewhat between the 18-month and 42-month follow-up interviews.

In both follow-up surveys, over one-third of the control group members reported that during the prior month they had drunk enough alcohol to feel high. Drug use was reported by about 10 percent of women in the control group. At the 42-month survey, almost 60 percent said they smoked cigarettes. These rates of substance use appear to be somewhat higher than those reported in a national survey of young adults aged 18 to 30 in 1990, although differences in question wording make exact comparisons difficult. In the national survey, only 7.7 percent of the women reported "heavy drinking" at least once a month, and 21.7 percent said they drank to intoxication once a month or more. Three percent said they used marijuana at least once a month. Only about one-third of the women in the national sample were smokers (Graves and Leigh, 1995).

Overall, the women in the New Chance control group had consistently high levels of depression, as measured with the Center for Epidemiological Studies Depression (CES-D) scale used at all three points of data collection in this study (Radloff, 1977).[11] At baseline, over half the control group (55.9 percent) had CES-D scores greater than 16, indicating they were at risk of clinical depression, and although depression scores declined significantly over time, 42.5 percent were still at risk of depression at the 42-month follow-up. A full 20.1 percent of the control group might be characterized as "chronically depressed"—that is, they had scores that placed them at risk of clinical depression at baseline and at both follow-up interviews.[12] Conversely, only 19.6 percent of the control group members were not at risk at any of the three data collection points.

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[9] Information on the pregnancy and birth rates was not available for TPD school dropouts who did not have a diploma or GED, the group most comparable to the New Chance sample.

[10] Cumulative data on pregnancy were not reported for the LEAP sample in Long et al. (1996). The LEAP researchers did, however, report a birth rate of 29.7 percent during the year prior to their three-year follow-up survey (for the out-of-school subsample, comparable to New Chance). In the New Chance control group, the comparable rate of repeat births during Months 25–36 was 21.7 percent.

[11] The CES-D consists of 20 statements such as "I had crying spells" and "I felt depressed." Respondents indicated how often in the past week the statement was true for them. The CES-D, which has been used in many studies of disadvantaged women (Belle, 1982; Coletta, 1983; Vega et al., 1986), has been shown to have excellent validity and reliability. In the present sample, the internal consistency reliability for the scale was .88 at the 18-month point (score range of 0 to 57) and .89 at the 42-month point (score range of 0 to 60). Scores on this scale can theoretically range from 0 (not at all depressed) to 60 (severely depressed). A score of 16 or greater is considered to be indicative of a level of depression that places the person at risk of clinical depression.

One-third of the controls reported at the final interview that they had felt highly stressed in the prior month. Very few young mothers reported that they had no one available as a social support, and most expressed satisfaction with their social support. Nevertheless, these young women had several ongoing problems and difficult circumstances that likely contributed to their stress and depression.

E. Employment and Earnings

As Table 3.1 shows, 36.7 percent of New Chance controls reported having had some employment in the year prior to their application. During the last year of the 42-month follow-up period, the annual employment rate had substantially increased. Fifty-one percent of the New Chance controls reported some employment, compared with 39.7 and 39.5 percent in the preceding years, respectively. As Figure 3.6 shows, monthly employment rates increased throughout the follow-up period, but these were substantially below the cumulative annual rates, suggesting frequent employment spells that were not sustained.

As employment rates increased over time, however, the proportion of this employment involving fewer than 30 hours of work a week decreased (from 41.7 percent in Month 18 to 28.9 percent in Month 42). Thus, it appears that while many control group members were employed only part of the years in which they reported employment, more of that employment was full-time.

Consistent with the increase in employment rates was a substantial growth in monthly earnings during the 42-month follow-up period. While average monthly earnings for controls were $129 in Month 18, they had increased to $274 in Month 42. Not all of this growth was due simply to an increase in the proportion who were working; those who were working on average earned more.

In spite of these positive developments, most controls did not earn enough to support their families. Among those who worked in the last year of follow-up, average earnings in that year were $6,030. Even among the 94 young women in the control group who worked in each of the last twelve months of follow-up, average earnings were only $11,126, or the equivalent of $927 a month. The fact that these earnings are not higher is mostly due to the low hourly wages sample members received when they were working. The average hourly earnings of controls who worked during Month 42 were only $5.89, up slightly from $5.56 at Month 18.

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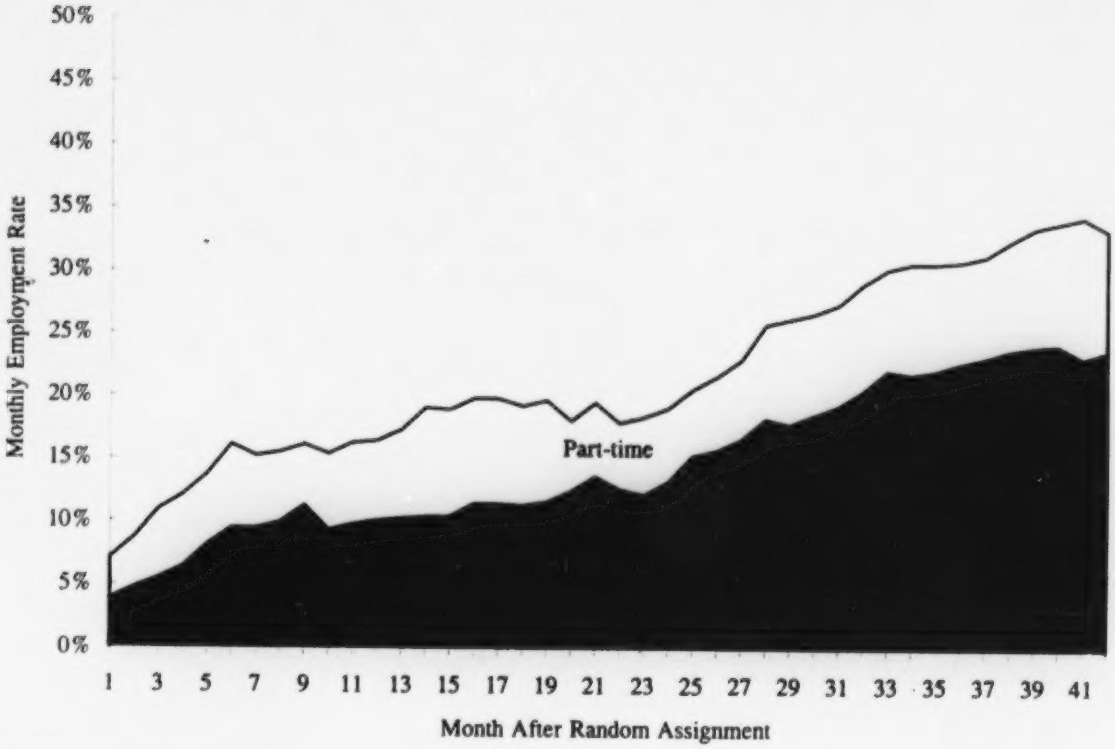
[12] Throughout this report, the term "depressed" is used to mean at risk of clinical depression as measured by the Center for Epidemiological Studies Depression (CES-D) scale.

Figure 3.6

Monthly Full-Time and Part-Time Employment Rates for New Chance Control Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.4 for data corresponding to figure.

NOTE: Part-time employment is defined as working fewer than 30 hours per week. Full-time employment is defined as working 30 hours per week or more.

How do these figures compare with earnings in other samples? In a survey of the 1994 U.S. population, 40.2 percent of single young mothers with children under age 6 reported being employed in the month preceding the interview.[13] This level of employment is higher than the employment rate among New Chance controls who were single, 31.5 percent of whom were employed during Month 42.

Also, the employment of young working women in the larger population was accompanied by substantially higher earnings than those received by New Chance controls. In 1994, the median weekly earnings for young working women (aged 16 to 24) were $276,[14] or 38.0 percent higher than the median of $209 earned by women in the New Chance control group who worked during Month 42.

Even in the absence of the program, however, New Chance control group members recorded employment outcomes that were comparable to or better than those found for their counterparts in other economically and educationally disadvantaged groups. For example, while 33.2 percent of New Chance controls were employed in Month 42, only 27.5 percent of JOBSTART controls with children were (Cave et al., 1993, p. 302), despite the fact that JOBSTART was more employment-oriented than New Chance.

It is also possible to compare employment outcomes for the New Chance control group members with those reported for teenage school dropouts in LEAP (Long et al., 1996, p. 75).[15] These young women in LEAP (the subsample most comparable to the New Chance sample) reported an employment rate of 26.4 percent in the three months before the three-year interview. The comparable figure for New Chance controls (combining follow-up months 34, 35, and 36) was substantially higher, at 34.4 percent. Differences in earnings were even more pronounced: while the out-of-school LEAP teens reported earnings of $362 for these three months, the New Chance control group members (including those who did not work at all) earned $749 on average.

Finally, New Chance employment rates can be compared with those for adult welfare recipients who were in the evaluations of the Job Opportunities and Basic Skills Training (JOBS) program, and Greater Avenues to Independence (GAIN), California's welfare-to-work program. Freedman and Friedlander (1995) found that 49.5 percent of the "Human Capital Development" group in the JOBS evaluation, a multisite study of mandatory welfare-to-work programs, was employed at some time during the second year of follow-up, compared with 42.2 percent of New Chance controls. In another study, Riccio, Friedlander, and Freedman (1994) found that 40.5 percent of experimentals in the GAIN evaluation were employed at some time during the second year of follow-up and 39.6 percent during the third year.

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[13] U.S. Bureau of the Census, 1995, p. 406. The source of this figure is the Current Population Survey (CPS). It was calculated by multiplying the reported labor force participation rate by one minus the reported unemployment rate for this group of women.

[14] U.S. Bureau of the Census, 1995, p. 433.

[15] Because of the mandatory and somewhat rural character of LEAP, this comparison is not entirely valid.

These comparisons show that employment rates in New Chance are surprisingly substantial, especially if one considers that both the JOBS and GAIN samples consisted mostly of older AFDC recipients, whose children were old enough to go to school. Also, the average sample member in the JOBS and GAIN studies was less disadvantaged educationally than the average New Chance sample member.[16]

F. Welfare Receipt and Family Income

Since many New Chance controls did not work or had low earnings, a substantial number remained on welfare continuously throughout the follow-up period. The solid line in Figure 3.7 shows that the monthly rate of AFDC receipt declined over time, but only marginally so. While shortly after random assignment virtually all control group members were receiving AFDC, at 42 months 73.9 percent continued to do so. The second (upward sloping) line in Figure 3.7 shows the cumulative percent of controls who left AFDC for a month or more during the follow-up period: 47.1 percent by Month 42. Thus, 52.9 percent of the control group received AFDC in every month of the follow-up period, but the percentage who left welfare at some time during the follow-up period increased from 26.0 percent in Month 18 to 47.1 percent at the end of the follow-up period. The graph suggests that this trend does not appear to be slowing.

Measures of family income were available for only the one month preceding each of the two follow-up interviews.[17] Between Month 18 and Month 42, reported average family income among controls increased from $793 to $1,150, an increase of 45.0 percent.[18] These averages mask much variation, however, in the amount, growth, and composition of family income. Figure 3.8 shows that the income distribution became more spread out over time. Much of the increase in average income appears to be driven by the top 25 percent of the income distribution. The 75th percentile of this distribution went from $858 at 18 months to $1,449 at 42 months, an increase of 68.9 percent. The median, on the other hand, increased by only 31.0 percent (from $642 to $841). Thus, it appears that some controls experienced a substantial increase in their income, while others experienced little growth.

The composition of family income changed also. Figure 3.9 shows that of monthly income reported by controls, the proportion that was accounted for by AFDC decreased from 41.7 to 24.5 percent between Months 18 and 42. At the same time, individual earnings became a more important source of income, growing from 16.2 to 24.0 percent of the monthly total. Similarly, the earnings of control group members' husbands and partners were a more important income component at the

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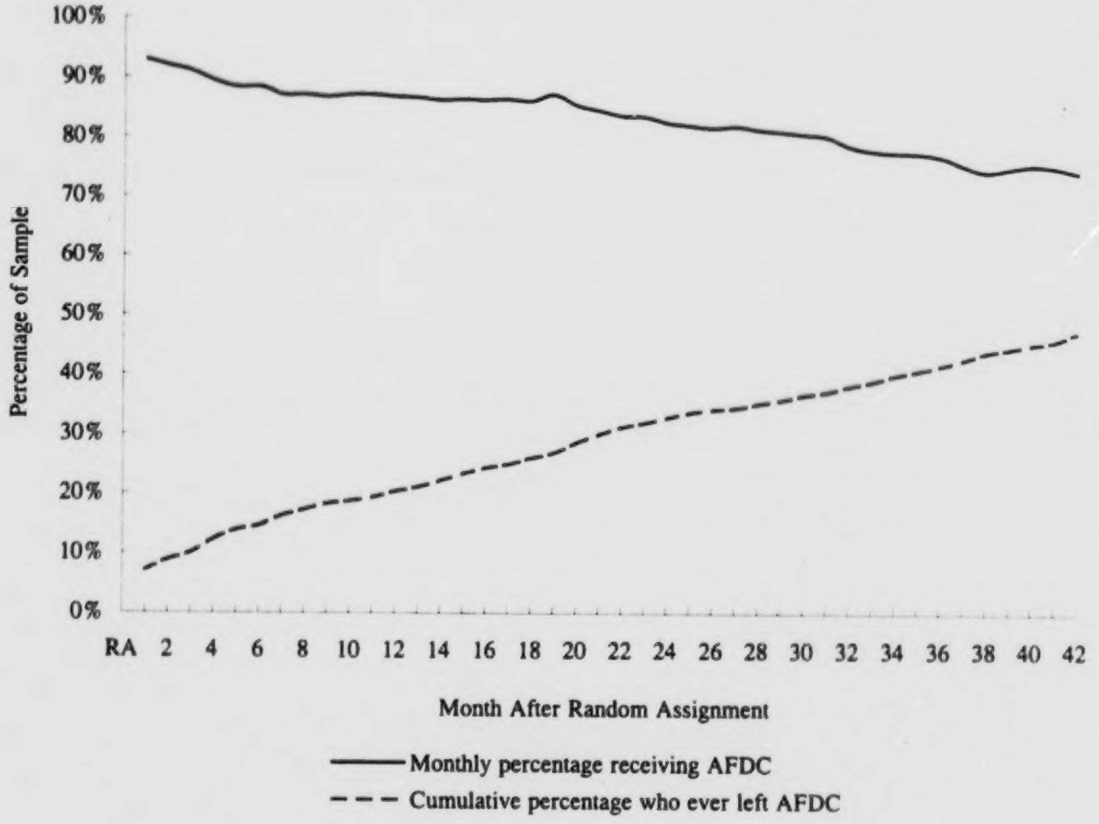
[16] Of the JOBS Human Capital Development group, 42.0 percent had a high school diploma or a GED certificate (Freedman and Friedlander, 1995). In the GAIN sample, this proportion was 50.9 percent (Riccio, Friedlander, and Freedman, 1994).

[17] Income measures were collected for an "economic unit" that included the sample member, her children living with her, and a husband or partner. The income of other household members and the financial benefits of it (except in the form of gifts) were excluded, as were housing benefits. Chapter 7 of this report provides a more extensive discussion of income measures.

[18] Note that the average size of the economic unit (the sample members, her children, and a partner or husband) for which income was collected grew from 2.9 to 3.3 persons between Month 18 and Month 42. Therefore, on a per capita basis the increase in income was only 27.5 percent.

Figure 3.7

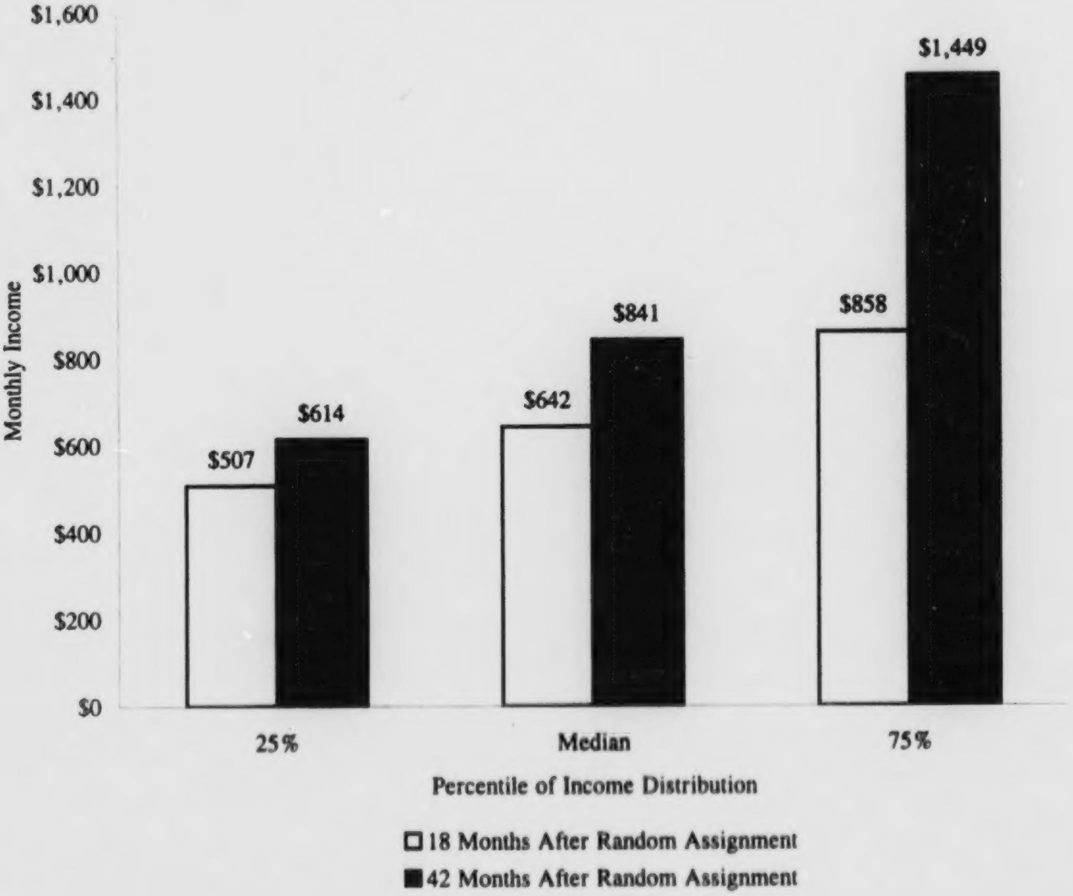
AFDC Receipt by New Chance Control Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.5 for data corresponding to figure.

Figure 3.8

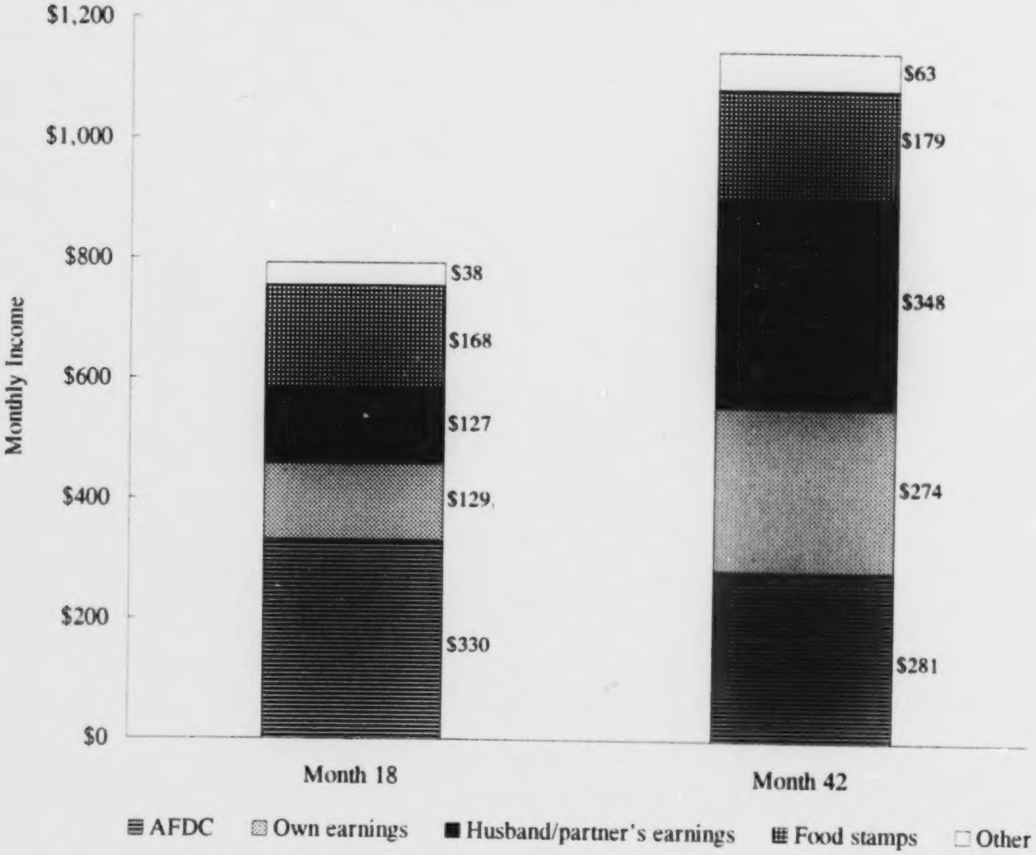
Monthly Income of New Chance Control Group Members at 18 and 42 Months After Random Assignment, by Percentile of the Income Distribution



SOURCE: MDRC calculations from New Chance survey data.

Figure 3.9

Income Sources of New Chance Control Group Members at 18 and 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.

time of the 42-month interview, having increased their share of average monthly income from 16.0 to 30.4 percent.

Figure 3.10 shows that differences in the income levels found for New Chance control group members were strongly associated with the types of income members had access to. The first two bars show that young women whose monthly income fell below the median were almost exclusively dependent on AFDC and food stamps. The difference in income between the lowest and the second quartiles was mostly accounted for by differences in the amount of public assistance received. For the third and fourth quartiles of the income distribution, this picture is very different. While AFDC and food stamps accounted for roughly half the total income reported by control group members in the third quartile, these two sources of public assistance accounted for only 14.9 percent of the income of controls in the top 25 percent of the income distribution. Unfortunately, these income figures cover only the month prior to the 42-month interview. As such, they do not reflect the instability that accompanies income from earnings, especially if they are someone else's.[19]

How did the New Chance control group compare with other, more representative samples in welfare receipt and family income? Among single mothers in the general population, the 1992 rate of welfare receipt was 36.3 percent (U.S. Congress, 1994, p. 1145), much lower than the annual rate of 85.7 percent found for the young women in the New Chance control group in the last year of follow-up. On the other hand, at 63.2 percent, the rate of welfare receipt among *poor* single mothers in the 1992 U.S. population was somewhat more comparable to that found for the New Chance control group.

Earlier in this chapter, New Chance controls were reported to have employment rates that were comparable to, or even exceeded, those found in samples of older welfare recipients. Comparing welfare receipt among New Chance controls with figures for their counterparts in the study of California's GAIN program and the JOBS evaluation, this favorable comparison does not extend to welfare outcomes. Of experimentals in GAIN, 73.4 percent were on welfare at some time during the last quarter of the first year of follow-up; that percentage dropped to 52.5 in the last quarter of the third year of follow-up (Riccio, Friedlander, and Freedman, 1994, p. 122). For New Chance controls, the comparable figures were 88.5 and 78.6 percent, respectively (not shown in tables). New Chance controls were also more likely to receive AFDC than were older women studied in the JOBS evaluation. In their account of JOBS impacts in three sites, Freedman and Friedlander (1995, p. 45) reported that 64.6 percent of experimentals in a Human Capital Development group received AFDC in the month prior to their follow-up interview, while 68.8 percent of controls did. The comparable figures for New Chance controls (covering Month 27, the month preceding the average month of follow-up in Freedman and Friedlander) was 81.7 percent. Thus, it appears that New Chance controls were more likely than their counterparts in other studies to report employment and AFDC receipt in the same month (unless

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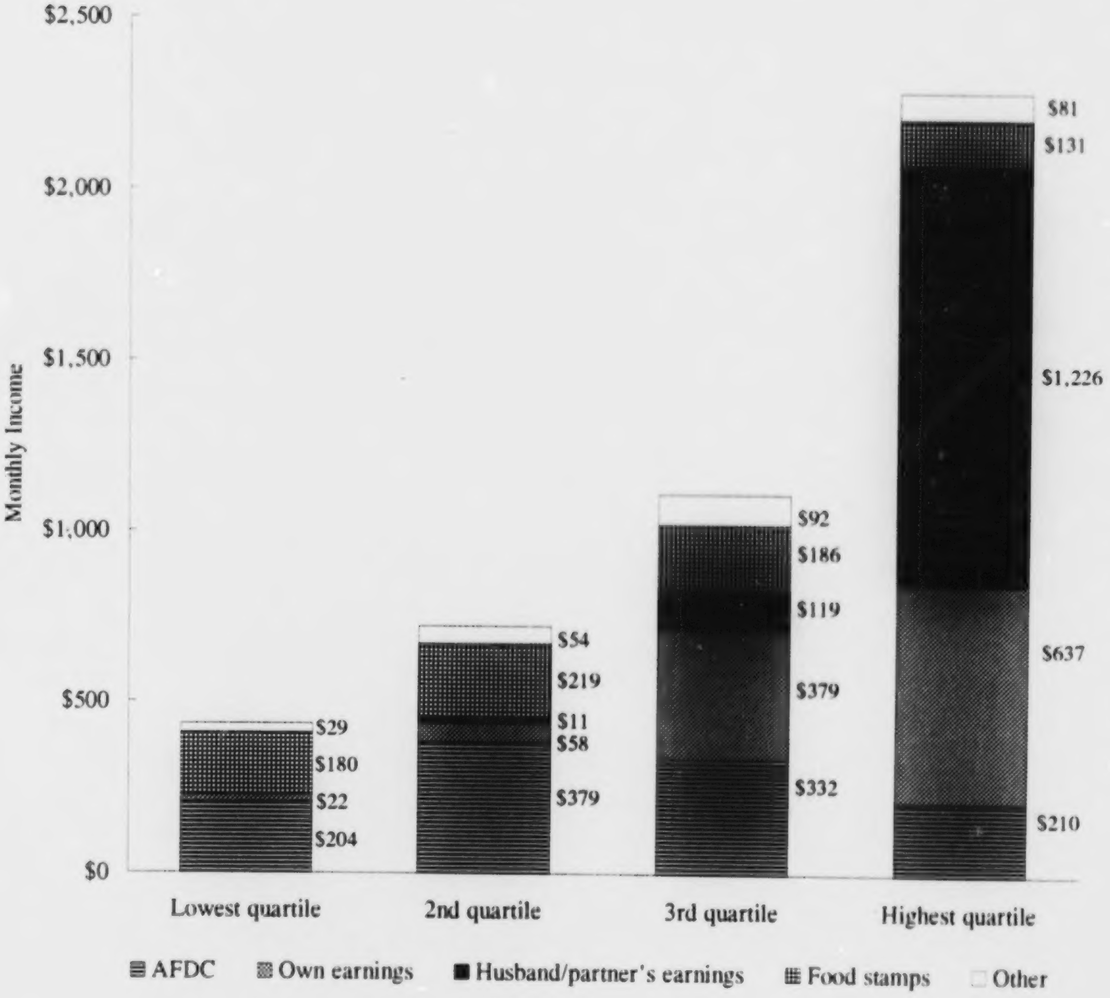
[19] It should also be noted that these income figures do not take account of transportation and child care costs associated with employment. Also, these figures do not include housing and medical benefits that may be tied to the receipt of AFDC.

Figure 3.10

Income Sources of New Chance Control Group Members at 42 Months After Random Assignment, by Quartile of Income Distribution



SOURCE: MDRC calculations from New Chance survey data.

many more sample members in GAIN and JOBS were neither employed nor receiving AFDC, which seems unlikely).

On an annualized basis, the median income of New Chance controls continued to be very low. At $10,090 (twelve times the median monthly income at Month 42), the median income projected for New Chance controls was less than half the 1993 median of $26,362 for all households in the United States.[20] Members of the control group who were employed in each of the twelve months of the last year of follow-up were projected to have a median income of $17,261.[21]

G. Parenting and Child Outcomes

The New Chance surveys captured many parenting and child outcomes with nationally normed scales such as the Home Observation for Measurement of the Environment (HOME) Scale (Caldwell and Bradley, 1984), the Bracken Basic Concept Scale (BBCS),[22] and the Behavior Problems Index (BPI). These scales and their statistical properties are discussed more extensively in Chapter 8 of this report, which is devoted entirely to parenting and child outcomes.

Data from the New Chance surveys uncovered a great deal of variation in the home environments of control group members' children.[23] From interviewer ratings, it appeared that 38.5 percent of the children lived in homes that were judged to be cluttered and messy, but 35.6 percent of the homes were rated "extremely clean." While 7.3 percent of controls' households were judged to be extremely noisy and chaotic, 54.0 were not noisy at all. And while 29.9 percent were described as dark and perceptually monotonous, 70.1 percent were "not at all" dark and monotonous. When asked to rate the safety of preschool children's play environment, interviewers assessed 44.2 percent of the homes as not "completely safe." About one-third of the mothers in the control group said that their neighborhoods were not good places to raise children, but another 27 percent rated their neighborhoods as very good or excellent in that regard. (The majority of families were living in apartments or row houses; 18.6 percent lived in public housing.)

There also was a lot of variation in the availability of cognitively stimulating materials in these families' homes. For example, at the final interview 25.8 percent of women in the control group reported getting a daily newspaper, despite their limited financial resources. On the other hand, most mothers in the control group (61.1 percent) said they never took their child to the library, despite the fact that more than half the women said they had a library card. About 40 percent of the mothers in the control group reported that the television was on in the home ten or more hours a day.

While some of these living circumstances may seem less than ideal, a comparison of HOME scores calculated from the New Chance survey data with 1986 HOME scores from the

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[20] U.S. Bureau of the Census, 1995, p. 432.

[21] This figure was calculated by multiplying by twelve the average family income for controls who worked in each of the last twelve months of follow-up.

[22] This cognitive scale is designed to capture the school-readiness of pre-schoolers. It is described in greater detail in Chapter 8.

[23] These measures refer to a single child in each respondent household (the "focal" child; see Chapter 2).

National Longitudinal Survey of Youth (NLSY) suggests that the quality of the typical home environment of New Chance controls compares favorably with that of poorly educated women in the NLSY.[24]

At the 42-month point, only 13.8 percent of the focal children in the control group were living in households that included their biological fathers. Among the children not living with their fathers, however, about 30 percent saw their fathers at least once a week. On the other hand, 27.7 percent had not seen their fathers at all in the previous two years, according to the mothers' reports. Almost 16 percent of the fathers were in jail, 2.6 percent were deceased, and the whereabouts of 17.9 percent were unknown to the mothers. The majority of children who were not living with their fathers had received no financial support from them in the two years prior to the 42-month interview (79.0 percent had no direct cash support, and 82.4 percent had no child support through welfare or child support enforcement agencies).

By the final interview, all children in the control group had been in some type of child care arrangement at some point in their lives. Fewer than half (41.0 percent) had been in a regular arrangement before their first birthday. Half the children (51.3 percent) had been enrolled in a day care center at some point, and 42.2 percent had been enrolled in a Head Start program.

At the 18-month follow-up, 52.6 percent of the children in the control group (who were, on average, 3.2 years old at that point), were in a regular child care arrangement. The rate of child care usage among New Chance controls at that time was similar to rates reported in the 1990 National Child Care Survey. In this national survey, about 54 percent of all children under age 5 were reported to have some type of regular nonparental care (Hofferth et al., 1991).

At the time of the final interview, 83.1 percent of the focal children were in a regular child care arrangement or in school. School was the most common arrangement (39.6 percent), followed by care by a grandparent (28.4 percent). While only 11.5 percent of the children were in a day care center at the 42-month point, nearly 23 percent of all mothers in the control group said that they had to pay out of their own pockets for some type of regular child care.

At both the 18- and 42-month interviews, about 80 percent of mothers in the control group described the focal child's health as very good or excellent. About 4 percent of the focal children, however, were reported by their mothers as having an ongoing health condition that made it difficult for the mothers to go to school or to work. Rates of injury, poisoning, or accident requiring medical attention declined as these children grew older—from 1.4 percent per child-month during the first (18-month) follow-up period to 0.8 percent per child-month during the period between the two waves of follow-up.

At the final interview 40.2 percent of mothers in the control group reported that the focal children had had something happen to them in the previous two years that was upsetting or disturbing. The most frequently cited traumatic experience was the mother's breakup or separation from her husband or boyfriend (17.4 percent), followed by the death of someone close (21.0 percent), a move to a new home (7.9 percent), and an argument between the mother and a male partner (7.1 percent). Some of the traumatic experiences (such as the loss of a pet) were ones that might happen to children in any environment, but others illustrate the extremely adverse circumstances of some of these families (for example, personal molestation, witnessing a relative being beaten up or stabbed, becoming homeless, being abducted, being shot).

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[24] This comparison is tentative, because the NLSY sample was more heterogeneous than the New Chance sample, a difference that may have affected interviewer ratings.

The vast majority of children had health care coverage (over 90 percent at both follow-up interviews), but 14.4 percent had had a period without any medical insurance at some point in the two years before the final interview—typically during a period when the mother went off welfare. About 70 percent of the children had been to a dentist in the year before the 42-month survey, but 22.4 percent had never been to one.

Children in the New Chance control group were, as a group, at a disadvantage in terms of their cognitive and socio-emotional development. As will be discussed in greater detail in Chapter 8, the New Chance survey effort included a standardized school-readiness test, the Bracken Basic Concept Scale (BBCS), and a number of questions on the children's behavior, forming a standardized Behavior Problems Index (BPI). On both of these measures, children in the New Chance control group scored worse than average, with a standardized BBCS score of 6.9, which translates to the 15th percentile nationally, and a standardized BPI score of 108.5 (0.56 standard deviations above the normed average of 100).

IV. Summary

This chapter has outlined three major points, each of which has important implications for the impact analyses presented in the remainder of this report. First, the description of baseline characteristics documented the educational and economic disadvantage of the average New Chance sample member on entering the study. Second, the analysis of outcomes over time for the control group showed how the young women in the sample improved their circumstances and experienced substantial personal growth, even without access to New Chance. In fact, in many aspects they did better than could be expected from other data sources covering similar groups of teenage mothers and welfare recipients. Finally, the 42-month interviews found that despite these positive developments, the average New Chance control group member was in poverty, on welfare, and not employed at the end of the follow-up period. Thus, while many sample members who were denied access to the New Chance program did relatively well on their own, many others did not.

The primary question for the chapters that follow concerns the extent to which New Chance was able to further enhance the positive developments this chapter documented for the control group. The program could have done so either by extending these developments to a larger share of the New Chance sample or by amplifying the longitudinal gains experienced by those who already were on a positive trajectory. The fact that controls did better than expected and had access to many education and training services, however, seems to limit the potential benefits of the New Chance intervention.

Chapter 4

Services and Service Receipt in the New Chance Demonstration

I. Introduction

This chapter first considers the nature and implementation of the New Chance program and the employment preparation and personal development services it provided. It then assesses differences between the experimental and control groups in the amount, comprehensiveness, timing, and quality of the services they received throughout the follow-up period. Finally, it presents information on the net cost of New Chance—that is, the difference in the cost of services incurred by enrollees and by their counterparts in the control group.

The discussion provides the context essential to an understanding of the chapters that follow, which constitute the core of the report and present the impacts of New Chance on education, fertility, employment, welfare receipt, child development, and other outcomes. To understand why any program does or does not produce the effects it sought to achieve, it is critical to consider four questions. First, are the kinds and amounts of services[1] called for in the program model capable of producing the desired effects? Second, are program services put in place as planned, and are they available in the prescribed amounts to all who are entitled to receive them? Third, do participation levels meet the expectations of the program's planners, so that enrollees receive the full extent and intensity of services that were envisioned, and if not, what factors make for participation rates that are lower than anticipated? Finally, do experimentals receive services that are significantly different from those received by controls? The last question is especially important because mostly voluntary programs like New Chance can be expected to generate impacts only if between-group differences in service receipt occur.[2] Such marked differences cannot be assumed in the New Chance evaluation, for five reasons:

1. New Chance did not have a monopoly on the services it offered. In every New Chance community, there were other programs and agencies from which young mothers could obtain services: local public school systems and community organizations offering free or low-cost adult basic education (ABE) and GED classes and community colleges offering a variety of vocational programs. Federal student loan programs also enabled students to take courses at community colleges or private vocational schools.

2. As was noted in Chapter 1, the New Chance sites received only limited additional funding to supplement the services they were already offering prior to the demonstration. Their

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[1] Throughout the report, the terms *component*, *service*, and *activity* are used interchangeably to refer to specific kinds of assistance—for example, adult education (ABE/GED), skills training, or counseling—called for by the program model.

[2] In contrast, mandatory programs for welfare recipients may have impacts on nonparticipants' employment rates and welfare receipt—that is, the length of time they remain on welfare and the size of their welfare grants—if the mandate induces them to seek and find jobs on their own, or if noncompliance with the program results in a reduction of their grants.

This discussion assumes that services are not stigmatizing to recipients, marking them as deficient or in need of remediation of some kind and thereby possibly reducing their attractiveness to potential employers.

ability to build in additional incentives (such as monetary stipends) to promote and reward participation was also limited.

3. Both experimentals and controls enrolled in New Chance voluntarily and were presumably motivated to go out and look for the services it offered. Denied access to New Chance, controls might well be expected to have sought these services from other sources.

4. Because young mothers were not mandated to participate in New Chance as a condition of welfare receipt, New Chance staff had no lever (such as the threat of reducing welfare benefits) to require experimentals to continue in skills training after completing a GED, to fulfill a minimum participation requirement, or, indeed, to participate at all; staff members could offer only inducements and the threat of termination from the program—an action they were often reluctant to undertake.

5. Participants faced many obstacles to regular attendance. Site staff identified a number of these obstacles: transportation problems, disruptions in child care arrangements, illnesses of the young women and their children,[3] conflicting welfare and medical appointments, and enrollees' lack of interest or lack of habituation to a daily routine. Personal problems, sometimes serious, also intervened. Reviewing the situations of early program entrants, program staff reported that almost half the young women with whose situations they were familiar did not have a stable place to live at some point during their program tenure and that smaller but disturbing percentages of enrollees were the victims of physical abuse, had alcohol or drug problems (or had family members or partners who did) that interfered with their program attendance, or were discouraged from participating by boyfriends or family members. The absence of an attendance mandate may have reduced participants' motivation to come to New Chance when they were feeling overwhelmed by these problems, and poor attendance, in turn, often led participants to fall behind and, ultimately, to drop out.[4]

A. Data Sources and the Structure of This Chapter

The information presented in this chapter is drawn from several sources: the New Chance Management Information System (MIS), the most detailed source of information on experimentals' in-program activities;[5] interviews with program coordinators and other key personnel concerning program operations; responses to questions in the 18-month and 42-month interviews eliciting experimentals' views of the program and information on service receipt by members of both groups; and data collected for the cost analysis.

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[3] Quint and Musick, 1994, suggest that pregnancy-related sickness and discomfort were another major reason for absenteeism.

[4] For further details, see Quint, Fink, and Rowser, 1991, pp. 105–10. According to New Chance staff members, enrollees encountered further difficulties when they moved into Phase II activities. The young women found college and training program instructors stricter and less supportive than New Chance staff had been, and the environments in general less congenial; they also had difficulty keeping up with the work or managing their time. These problems also led to absenteeism.

[5] The MIS is, however, incomplete as a source of data on *all* the activities in which experimentals took part. First, it excludes post–New Chance activities and others of which program staff were unaware. Second, time spent in individual counseling was not recorded on the MIS, since counseling sessions were often informal and unscheduled. Third, the MIS does not include hours of college attendance or job placement assistance. Finally, it includes time spent in health education classes but not receipt of health care services.

Section II of the chapter describes the program structure, the staff complement, and the services delivered in Phases I and II of New Chance. That discussion is followed by data on the extent to which experimentals received these New Chance services and a comparison of the amount, comprehensiveness, and timing of service that experimentals and controls received, along with their opinions of these services as a proxy for measuring service quality. The net cost of New Chance is then considered, and comparative data are presented on service receipt in other demonstrations. The final section discusses the implications of the preceding data for the impact analysis chapters that follow.

B. A Preview of the Findings

Essentially, the chapter concludes that the Phase I components of New Chance were put in place as planned and that staff were successful in creating an atmosphere that enrollees perceived as caring and supportive. At all sites, enrollees spent about half the day in ABE and GED classes; the other half was divided among other employment preparation and personal development services. Some services (for example, the education and parenting classes) proved much easier to implement than others (such as employability development and family planning counseling); however, every site had at least one exceptionally strong component. Implementation of the Phase II components varied considerably from site to site.

The large majority of experimentals (88.5 percent) participated in the program, and their survey responses indicated that they liked New Chance and believed they had benefited from its services. Because of high absenteeism, however, and because many of the young women dropped out of the program relatively early, participation in all activities was much lower than anticipated, and the treatment was also much briefer.

That many enrollees received much-attenuated services is likely to have undercut the program's potential to produce positive impacts; experimentals may not have received enough of some services to make a difference, especially personal development services, such as parenting education. These services were scheduled for relatively few hours to begin with, and absenteeism reduced their receipt still further.

A second factor reducing the likelihood of detecting impacts is the high level of service receipt by controls. Compared with controls, experimentals received more of almost every type of program service over the 42-month period, received more different kinds of services, and received them more quickly. But the majority of controls also received services, especially ones designed to help them prepare for work. Thus, for example, 95 percent of the experimentals attended ABE, GED, or college classes, skills training, or a job club during the follow-up period; so did 86 percent of the controls. The difference in levels of service receipt between the two groups was most pronounced during the first 18 months of follow-up—although it was much smaller than program planners desired or anticipated—and diminished over time, as experimentals left New Chance and controls entered other education and training programs. Indeed, during months 19–42 of the follow-up period, controls were significantly more likely than experimentals to participate in ABE/GED classes, and during this period the two groups received essentially identical amounts of all personal development activities except counseling.

The evaluation, then, may be seen as testing the impacts of two different service *packages*, rather than comparing the receipt of services with no services. The question it addresses is whether getting a somewhat larger, more comprehensive group of services, and getting them several months earlier than controls, was enough to make a positive and lasting difference in experimentals' often tumultuous lives.

The total average net cost of New Chance over the 42-month period (including costs of services received by experimentals after they left the program) ranged from $6,197 to $7,443 depending on the method of calculation. (Net costs are program costs minus the costs of services received by controls.) This cost varied a good deal among the New Chance sites. Three services accounted for the majority of net costs incurred by experimentals: adult education (ABE/GED classes) ($671), case management (ranging from $1,242 to $2,474), and child care (ranging from $2,271 to $2,287).

II. The New Chance Structure, Staff, and Components

A. The Program Sites and Sponsor Agencies

New Chance was implemented in a variety of contexts. The 16 New Chance sponsor agencies or program operators (listed in Table 1.1) were located in different areas and served different populations. The Bronx, Detroit, Harlem, Philadelphia, and Pittsburgh sites all might be characterized as "inner-city," drawing enrollees mostly from nonwhite, high-density poverty areas. Other sites enrolled participants from a much larger geographical area. The Chicago Heights site, for example, drew its enrollees from a sprawling area of largely working-class and poor suburbs south of Chicago; the Denver program attracted participants from all over the metropolitan Denver area.

As Table 1.1 shows, the sponsor agencies also were different kinds of institutions—community service organizations, schools and a community college, a Private Industry Council (the nonprofit entity that administers funding under the federal Job Training Partnership Act, JTPA), and a county government agency—which, at the demonstration's outset, had varied missions. Interestingly, while sites differed in the extent to which MDRC staff viewed them as giving greater emphasis to one or the other of the twin objectives of New Chance—the young women's preparation for employment and self-sufficiency or their personal development and acquisition of parenting skills—these emphases did not necessarily parallel the sites' original missions.[6]

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[6] While half the sites were judged to emphasize both goals equally, four (Allentown, Chula Vista, Minneapolis, and Salem) were deemed to place greater emphasis on parenting and personal development goals. Interestingly, at two of these sites (Allentown and Salem), improving parenting skills had been a principal mission of the teen parent programs operated by the sponsor agency prior to New Chance; the other two sites had focused on human capital development as their chief aim prior to New Chance. Four sites (Harlem, Jacksonville, Philadelphia, and Pittsburgh) were judged to give greater emphasis to self-sufficiency objectives; at three of these sites, self-sufficiency had also been a strong thrust of the sponsor agency before New Chance, but the fourth (Jacksonville) had been mainly geared toward family planning and reproductive health issues.

Although the sponsor agencies selected to run New Chance were all experienced program operators, about one-third of the sites had never before managed a program specifically targeted for young mothers; the other two thirds had largely served in-school youth and non-AFDC recipients. Mounting a new and comprehensive program thus posed many challenges.

B. The Staff Complement

While the sites were generally successful in assembling a complement of experienced staff members who strongly supported the program's objectives and who related easily to participants, they varied considerably in their staffing patterns. All sites had a core staff, which included a New Chance coordinator and one or more case managers, but they adopted different methods of filling the remaining positions. For instance, at some sites, ABE/GED instructors were employees of the sponsor agency, while at others they were provided to the site through an agreement with the local school system, which remained their official employer. The number of New Chance full-time equivalent staff positions ranged from 3 in Denver to 16 in Portland and 17 in Pittsburgh. Different staffing configurations had implications for program costs, which are detailed later in this chapter.

A survey of staff members was conducted relatively early during the demonstration period and offers a profile of the staff at that time. Despite staff turnover (which varied from site to site and averaged about 20 percent from the start of random assignment in August 1989 through April 1991), the general characteristics of the staff remained relatively stable.

New Chance was staffed overwhelmingly by women; two thirds of the sites had all-female staff. Three quarters of the staff members were in their thirties and forties. Three quarters had children, and 62 percent of those with children had children in their teens or older.

About half the staff members had received bachelor's degrees, and an additional 30 percent held master's degrees. Almost 90 percent reported having previously worked with teens or young adults, in many cases extensively: 32 percent for ten or more years, and 45 percent for three to nine years. A considerable majority of the staff who had worked with teens had worked with pregnant or parenting teens before New Chance (70 percent of all staff).

Overall, Hispanic and black staff were underrepresented relative to the percentage of Hispanic and black enrollees in New Chance. While the racial and ethnic make-up of the staff might be thought to be an important factor influencing the ability of participants to bond with staff and the ability of staff members to serve as role models for the young women, there was no significant difference in staff perceptions of their rapport with participants based on race or ethnicity.[7] Across all sites, 91 percent of staff members rated their relationships with participants as "good" or "excellent" (ratings of 4 or 5 on a 5-point scale). The remaining 9 percent gave their relationships a "fair" rating. Similarly, 65 percent of the staff members felt that only rarely or occasionally were participants not candid with them.

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[7] Unfortunately, corresponding data on participants' ratings of their relationship with staff members are not available.

C. The Service Sequence

Although not required to do so by the program guidelines, all sites except one (Portland) elected to operate New Chance as a program in which ABE/GED and vocational skills training took place sequentially rather than concurrently.[8] The program was therefore divided into two distinct phases. The Phase I components—adult education (ABE/GED classes), employability development (career exploration and pre-employment skills training), workshops in life skills, health, family planning, and other personal development areas, and parenting classes—were generally delivered on-site, for approximately six hours a day, four or five days a week. Participants could remain in this phase of the program for five months; if they had not earned a GED by that time, they were supposed to enter a work internship (that is, work experience) position or another activity more directly oriented toward work. The work experience and occupational skills training positions that made up Phase II were usually off-site.

Several factors led most sites to arrange education and training sequentially. First, the desire to incorporate parenting and personal development along with education into the first phase of the program left less time for other services. Also, the young women were believed to need strong educational preparation to succeed in skills training; in fact, many skills training programs would not accept applicants who did not have a high school diploma or GED in hand or would not allow them to complete or advance within the courses.

The following sections describe the Phase I and Phase II components. It should be stressed, however, that New Chance was more than an assemblage of services. Central to the program were the *relationships* enrollees formed with staff members and with one another. New Chance was intended to be completely different from the often impersonal, anonymous high school environments in which many enrollees had previously experienced failure, and in this respect it was, according to participants' ratings (discussed later in this chapter), a marked success.[9]

D. Implementation of Phase I Activities

A detailed analysis of early program operations appears in Quint, Fink, and Rowser (1991), where the issues involved in establishing the infrastructure, recruiting participants, and providing Phase I services receive extended discussion. A major finding of that report was that the 16 demonstration sites implemented the early program components relatively uniformly, generally offering the required hours of each service prescribed by the program guidelines (see Table 4.1).[10] Moreover, whatever the initial orientation or philosophy of these agencies, some services (for example, adult education and parenting) generally proved much easier to implement than others (for example, employability development and family planning counseling).

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[8] The New Chance program in Portland was operated jointly by the Portland Public Schools and the Job Corps and adopted a program model in which skills training took place concurrently with other program activities.

[9] Programmatic and spatial separation were among the factors making for a sense of closeness and separate identity. New Chance enrollees attended most Phase I activities apart from other clients of the sponsoring agency, often in a separate physical location—a series of rooms or a separate building.

[10] No site's schedule exactly matched the participation requirements in the guidelines, however, because of pragmatic concerns (space or staff availability), convenience, or the belief that a component warranted more (or less) attention than the guidelines specified.

Table 4.1

Schedule of New Chance Phase I Components, by Site

Site	Days of Program Operation per Week	Scheduled Hours per Week: Adult Education (ABE/GED)	Scheduled Hours per Week: Employability Development	Scheduled Hours per Week: Health and Personal Development: Life Skills	Scheduled Hours per Week: Health and Personal Development: Health Ed.	Scheduled Hours per Week: Parenting Education	Scheduled Group Sessions per Month: Family Planning	Scheduled Individual Meetings per Month: Case Management[a]
Allentown	4	10	2	1.5	2	1.5	2	2
Bronx	5	15-20 [b]	3	2.5	2	3-4 [b]	1-4 [b]	1
Chicago Heights	4	12	3	1.5	3	3	2	2
Chula Vista	5	20	2	3	2	2	2	1
Denver	5	10	4	1.5	4	9	4	3
Detroit	4	12	2	1-2 [b]	2	1.5	1	1
Harlem	5	15	1.5	1.5	1.5	1.5	4	1 [c]
Inglewood	5 [d]	10	8	3	1.5	1.5	1	1-2
Jacksonville	5 [d]	14.5	5	1.5	1	2	1	2
Lexington	4	13	2	2	2	3	1	2
Minneapolis	5	12.5	2.5	2.5	2.5	2	-- [e]	2-3
Philadelphia	5	13.5	1.5	2	2	2	2	1
Pittsburgh	5	6.5-19 [f]	2-9 [f]	1.5	2	1.5	1	4
Portland	5 [d]	10-15 [g]	5	3	1	2	1	2
Salem	4	12	3.5	3	1.5	3	1	-- [h]
San Jose	5 [d]	13	3	1.5	2	3	2	-- [h]
Guideline requirements	4-5	12-15	48 hours before work internship or skills training	18 90-minute sessions	1.5	2-4	1	2

(continued)

Table 4.1 (continued)

SOURCES: New Chance site data and staff interviews.

NOTES: Several services provided by New Chance programs are not included in this table: adult survival skills training, which at some sites was scheduled as a separate class and at others was incorporated into other components; occupational skills training, which was offered during Phase II of the program (except in Portland and Harlem, where participants attended occupational skills training concurrently with the adult education, health and personal development, and employability development components); and other group activities such as group meetings or field trips.

[a] Case management was provided in Phase II as well.

[b] The number of component hours varied, depending on the weekly program schedule.

[c] At this site, case managers originally scheduled three to four meetings per month, but as the number of participants increased, case managers reduced the number of scheduled meetings.

[d] On Fridays, only adult education classes were scheduled.

[e] The site operated on a module system, devoting a one-week module to family planning.

[f] Participants were placed in one of four class levels depending on their educational ability: pre-GED 1, pre-GED 2, GED, or career orientation. The number of hours a participant spent in adult education and employability development activities varied depending on the class level.

[g] Depending on their Tests of Adult Basic Education (TABE) results, participants were required to attend either two or three hours of adult education classes per day.

[h] Case managers met with participants informally, but no formal case management meetings were scheduled.

Brief descriptions of the individual Phase I activities follow. (Although participants were entitled to free child care throughout their participation in the program, this component is also discussed below.)

1. **Recruitment**. Recruiting participants required ongoing effort. At the outset of the demonstration, the local programs lacked a track record, and many sites had little prior experience publicizing their services or recruiting to fulfill goals or meet timetables.

Almost all sites relied heavily on the local welfare agency to identify and conduct outreach to potentially eligible young women. Welfare agency staff helped the New Chance sites with recruitment by sending mass mailings of program flyers, referring individual clients, and scheduling sessions at which New Chance representatives described the program to prospective enrollees. Other approaches used by the sites included publicizing New Chance through various community agencies and through the media, holding an "open house" for prospective enrollees, recruiting door-to-door, and having current participants recruit other young mothers.

2. **Education**. The New Chance guidelines required that the education component include adult basic education (ABE)—reading, writing, and basic mathematics—and an introduction to and preparation for the GED test, which covers a number of academic subjects. Also required was an introduction to computers and their applications, in sites where computers were available. The widespread availability of GED curricula and of teachers experienced in teaching GED or adult basic education classes, along with the fact that most sites were already running such classes when New Chance was introduced, made it relatively easy to operate an education component.

Participants' own interest in getting a GED also contributed to the relative ease with which the component was implemented. Indeed, the opportunity to earn this certificate was the major reason many enrollees gave for joining New Chance, and they spent more time in ABE/GED classes than in any other program service.

Most sites emphasized individualized instruction. Students were assessed for their skill levels in math and reading and then spent the majority of their time working on their own, with a workbook or text geared to their academic level and with instructors available to answer questions and work one-on-one with students. Over time, computer-assisted instruction was used to supplement more traditional materials. Staff reported that finding adequate instructional materials and motivating participants with low reading or math skills was difficult; the young women needed constant reinforcement and reassurance that they were improving.[11]

3. **Employability Development**. Employability development combined career exploration with pre-employment skills training. The focus of career exploration was on acquainting participants with various fields and careers, helping them understand the duties, education prerequisites, and skills requirements of a range of jobs in which they expressed interest and exposing them to people actually doing those jobs. The pre-employment skills class covered

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[11] In retrospect, such techniques as cooperative learning and peer tutoring might have engaged students more fully than the highly individualized instruction that was provided.

job search techniques, applications, resumes, appropriate dress for the workplace, employer expectations, and job-keeping strategies.

Employability development was difficult to implement well, for many reasons. Often the person responsible for teaching it was also responsible for placing participants in skills training or work internships. (In other instances, case managers were responsible for skills training and work internship placements along with their other responsibilities.) Thus, the position of employability development instructor required a wide range of skills and capabilities, and sites found it hard to find individuals who could do all these things well. Smooth functioning of the component was also hindered by sponsoring agencies' inexperience with this service and by difficulty in finding suitable curricula.

Finally, along with the problems program operators experienced, staff also reported that many participants were not very motivated to enter skills training or jobs. Their primary objective in the program was to pass the GED test, and they did not plan far beyond that immediate goal. Furthermore, staff described many program entrants as unrealistic in their expectations, knowing little about the skills various occupations require or expecting to be able to get a good job with just a GED in hand. Changing attitudes such as these was a challenging task.

4. **Parenting Education**. The goal of parenting classes was to help participants cope better as parents and to foster their children's cognitive, social, emotional, and physical development. Parenting, like ABE and GED instruction, was a fairly easy component to implement, even for sites that did not start with a focus on it, partly because MDRC trained parenting instructors with a curriculum designed for use with disadvantaged young mothers. The curriculum included units on child development, developing values in children, preventing child abuse, nonsexist childrearing, addressing children's medical needs, and dealing with accidents and emergencies.

The guidelines for parenting classes discouraged lectures and encouraged active, participatory sessions; most classes involved considerable discussion. A major focus of the component was on the developmental stages of childhood, so that participants could better recognize and understand age-appropriate behavior, learn how to stimulate their children's mental and physical development at different stages, foster their self-esteem, and use appropriate discipline. Staff reported that participants were interested in learning how to accomplish specific goals such as toilet training but that many enrollees did not perceive a need for parenting classes, seeing themselves as good mothers already. Staff also noted that the participants were often initially mistrustful of the parenting instructor, that enrollees' family members were sometimes unsupportive of the parenting skills New Chance tried to teach (such as the use of "time-out" rather than disciplinary methods with which they were more familiar), and that emotional problems, such as low self-esteem and depression, interfered with a participant's ability to improve her parenting skills—or, indeed, to resolve any of her problems.

On average, students were scheduled to attend parenting classes for two hours a week. Thus, a student who was enrolled in Phase I activities for five months would receive about 42 hours of parenting instruction over the course of her stay, if she were present every day (as few participants were).

5. **Life Skills**. Life skills in New Chance comprised two elements. Life Skills and Opportunities (LSO), a separate class consisting of 18 90-minute sessions and built around a curriculum especially developed for the demonstration, was designed to foster skills in decision-making, effective communication, assertiveness, problem-solving, contingency planning, and working in groups. It addressed these skills in the context of four main areas of participants' lives: sexuality, relationships, parenting, and the world of work. Although these areas were also covered in other components, the objectives of LSO and the structure and format of the classes (which concentrated on participants' involvement in structured activities such as role-playing and discussions of their ideas and feelings) differentiated it from the other classes. Staff members agreed that participants' favorite topics included sexuality, male-female relationships, and assertiveness; they were less responsive to the sessions on breaking stereotypes and combining work and family.

Several instructors felt that the curriculum was useful but also believed that teenagers tend to "live for the moment" and consequently were unlikely to change their decision-making habits immediately. These instructors hoped, rather, that the component would teach participants skills they might put into practice at a later point, even if they did not immediately appreciate their relevance.

The second life skills element, known as adult survival skills, was not a separate class but consisted of a variety of practical skills that were to be included in the curricula of other components. The guidelines defined adult survival skills as consisting of the following areas: money management (e.g., budgeting, banking, taxes, and credit), transportation (e.g., reading maps and schedules), time management (e.g., using alarm clocks, making schedules, setting priorities, and establishing contingency plans), getting information and personal records (e.g., using public agencies, libraries, and newspapers), rights and negotiations (e.g., knowing about anti-discrimination laws, child support, voter registration, and unemployment insurance), and interpersonal skills (e.g., assertiveness, effective use of the telephone, and conflict resolution).

6. **Family Planning**. An important objective of New Chance—one with which staff strongly concurred—was to provide participants with the knowledge and services needed to postpone further childbearing and to assist them in developing the motivation to do so until they were in a better position to provide for their families. The program's family planning component included education classes or workshops, individual counseling, and linkages with family planning service providers. The guidelines specified that classroom instruction should be provided by trained staff at least once a month (with two additional classes to be scheduled during orientation), that approved curricula should be used, and that case managers should counsel each participant regularly on her family planning practices.

The monthly classes on family planning generally proceeded as planned. The existence of public and private agencies specializing in health and family planning made linkages with such organizations an appealing option for obtaining trained staff to lead the classes. A major issue for the program as a whole, however, was that at a number of sites case managers did not routinely or effectively counsel participants about their use of contraceptives. Some case managers resisted this role because they were uncomfortable dealing with the subject of sexuality or felt that they lacked the requisite expertise. Still others were comfortable with the subject but, given the limited time they had to spend with each participant, tended not to discuss family planning unless the young woman raised it as a specific problem.

The family planning part of the program posed difficult challenges, because staff had not only to provide participants with knowledge about contraceptive methods but also to instill in them the motivation to use these methods regularly. Staff members noted that changing a participant's behavior was often complicated by the presence of a new partner in her life and that many participants were reluctant to insist that their partners use condoms.

7. **Health Education and Services.** Participants' attitudes and behavior affected not only their own health but also that of their children. The health component sought to help participants develop healthier lifestyles by emphasizing the importance of preventive care and by facilitating their access to health care services. Health education was provided on-site, while services were available through hospitals and clinics and, at three sites, at on-site health care facilities.

As prescribed by the guidelines, the health education segment included weekly 90-minute classes covering a wide range of topics: AIDS, the use of community resources, children's illnesses and immunizations, physical and emotional abuse, women's reproductive health issues, and the role of preventive care. Because their subject matter overlapped, health classes were commonly integrated with parenting and family planning classes. At some sites, depression and stress management were also discussed, and some programs offered classes on cardiopulmonary resuscitation (CPR). In addition, information about drug and alcohol abuse was conveyed. At some sites, staff members reported that some participants had substance abuse problems; at others, they indicated that substance abuse was a common problem among participants' family members or partners.[12]

8. **Case Management.** New Chance case managers had multiple responsibilities, including orienting new enrollees to the program, assessing participants and monitoring their progress, counseling them, advocating on the young women's behalf, and keeping records. Case conferences, at which all staff discussed certain participants, helped case managers ascertain participants' progress in the program components. When a participant was absent, the case manager or another staff member called her, usually that day or the day after. In addition, three fourths of all case managers also taught one or more classes in New Chance and/or were responsible for recruiting new enrollees.

More than half the sites did not adhere to the guidelines' requirement of scheduled biweekly meetings with each participant. Especially as their caseloads increased, many case managers felt that they had too many other responsibilities to adhere to a formal schedule of meetings. Case managers at all sites met with participants more often on an informal than on a formal basis, and participants often visited their case managers during lunch hour or breaks simply to chat. Crises in participants' lives necessitated additional and often lengthy meetings with the case manager. Case managers reported that, in addition to counseling participants in crisis situations, they also spent time with participants who were "having a bad day" and needed someone to talk to.

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[12] Portland arranged for counselors from another community-based agency to provide drug and alcohol education, individual assessments, and, if necessary, referral for treatment.

At the majority of sites, rising enrollments meant that at some point caseloads exceeded the maximum recommended size (specifically, no larger than 25, and no larger than 15 if case managers had other responsibilities). At most sites, one or two staff members were assigned as case managers; as enrollment increased, caseloads often rose into the 40s and, at a few sites, to as high as 60 or 70.[13] High caseloads, coupled with other program responsibilities, forced many case managers to rely on informal meetings with participants, to delay contacting absent participants, and to reduce other monitoring and record-keeping activities.

9. **Child Care**. Free child care is a key element of the New Chance model, intended both to facilitate the young mothers' regular participation in program services and to enhance the development of their children. Regular child care was provided to participants' children at the program site in 9 of the 16 program locations.[14] A few sites, notably Inglewood and Jacksonville, made arrangements for the children of New Chance enrollees at family day care homes or day care centers located close to the program facility. Some sites used resource and referral agencies to help participants locate care, while many participants found care on their own.

Because child care was considered an important mechanism for enhancing the development of the New Chance children, the quality of child care provided to them was assessed as part of the research. Data for the assessment were gathered in 8 of the 16 sites; the sample included 7 on-site centers and 4 off-site centers. Measures of the quality of child care provided in these centers were derived from two sources: a survey completed by the center directors covering structural features that research has shown to be correlated with positive child development outcomes (for example, group size, child-to-staff ratio, training and education of the caregivers, and staff stability); and ratings of the overall quality of the child care environment based on observation by MDRC staff using observational instruments that have been employed in large-scale day care studies.[15] Quality was assessed in two ways: by whether the centers met established standards of quality care and by comparing the quality of care with that in typical centers used by low-income populations.

The New Chance child care centers generally met the standards for group size and child-to-staff ratio established by the National Association for the Education of Young Children (Bredekamp, 1984). Subsamples of centers from two other recent studies—the Profile of Child Care Settings (Kisker et al., 1991) and the National Child Care Staffing Study (Whitebook, Howes, and Phillips, 1990)—provided comparison samples of centers primarily serving low-income children. The New Chance centers proved as good as or better than these other centers on most dimensions.

In general, then, the child care assessment indicated that the New Chance child care centers offered moderately good-quality care, demonstrating the feasibility of integrating good child care into programs for young welfare mothers. The fact that child care provided by New Chance centers was not top quality, however, raises questions about whether the care was good enough to improve the development of seriously at-risk children, who may need the best quality programs (Fink, 1994).

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[13] At any time, some percentage of the caseload was inactive. Nevertheless, case managers often spent time working with these women, keeping in touch with them and encouraging them to return to the program.

[14] Two sites (the Bronx and Philadelphia) offered temporary care only; a third site, San Jose, had an on-site center, but it was used by few New Chance enrollees because slots were not set aside for them.

[15] The Infant and Toddler Environment Rating Scale, or ITERS (Harms, Cryer, and Clifford, 1990), was used to obtain quality ratings in infant and toddler classrooms. The Early Childhood Environment Rating Scale, or ECERS (Harms and Clifford, 1980), was used to measure overall quality in preschool rooms. Two MDRC staff were trained to use these scales. Inter-rater reliability was quite high, at least 90 percent, for both scales in practice sessions.

10. Other Activities. Over time (and sometimes after the formal demonstration period ended), sites expanded their service offerings beyond those mandated in the guidelines to respond to other needs of participants. Pittsburgh, for example, included driver education classes, and other sites added exercise classes. In a few cases, staff wanted to increase the attention paid to a particular topic within a component (for example, drug and alcohol abuse) and developed a separate class on that issue.[16] Some sites scheduled group meetings on a regular basis to resolve problems among participants, to discuss program rules, or to plan an event; at other sites, these sessions were held only on an as-needed basis.

11. Putting It All Together: A Typical Day in the Program. A description of a representative day in the program shows how the various parts of the program model were intended to come together to form an integrated whole.

Classes started at 9:00 A.M. at the majority of sites but as early as 7:30 A.M. at some sites. A participant whose children were cared for by a relative or a friend outside her home stopped at the provider first to drop her children off on her way to the site; participants whose children were cared for in on-site daycare brought their children with them. Participants at sites that did not provide transportation generally took public transportation or found someone to give them a ride. Some sites provided breakfast, and participants who arrived on time ate breakfast with other participants and often with one or two staff members as well, sharing personal news, talking about local places and events, relating stories about their children, and chatting about other topics of interest.

Both by design and because of absenteeism, classes were usually small, with perhaps 20 students or fewer in attendance on any given day. The daily schedule usually called for ABE/GED classes in the morning and other activities in the afternoon (or, less commonly, vice versa). In the education classes, students typically sat at small tables, two or three students to a table. Although they usually worked on separate assignments, they frequently consulted each other, asking, for example, how to spell a word or do a math problem. The teacher usually circulated around the class, keeping students on task, offering help when needed, and dispensing praise and encouragement. Topics raised in one class were sometimes explored further in another; for example, students might practice their writing skills by writing an essay on a subject discussed in parenting class.

At lunchtime, a participant could check in on her child in the on-site child care center, eat with other participants, or spend extra time on academic preparation. After-lunch activities—health, parenting, and especially LSO classes—often involved participants in lively discussions about values and attitudes and in entertaining role-plays of the kinds of situations enrollees were likely to confront. Participants sometimes disagreed with each other, but teachers and students maintained an atmosphere of mutual respect. Classes usually ended between 2:30 and 4:00 P.M.

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[16] Other program coordinators, in "wrap-up" interviews at the conclusion of the operational phase of the demonstration, noted the need for a separate component dealing with substance abuse.

E. Implementation of Phase II Activities

The activities that made up the second phase of the New Chance program model—occupational skills training, work internships, and job placement assistance—are of particular importance, because these are the components that most directly prepared young women for employment. Phase II components differed from the Phase I components in several respects. First and foremost, most Phase II activities were usually off-site. This shift in locale meant not only that enrollees had to exchange a familiar and congenial setting for one that they often perceived as less welcoming, but also that program staff had to spend a good deal of time interacting with outside agencies in order to set up Phase II placements and to monitor the young women's performance in them. Second, whereas all enrollees in Phase I received essentially the same services (except for case management, which was individualized), Phase II activities were tailored to suit the needs and skills of each participant. Finally, while Phase I activities tended to be relatively uniformly implemented from site to site, there was considerable variation in the extent to which sites offered the Phase II services. (Data on participation in Phase II activities are presented in the following section.)

The transition to Phase II was often difficult for both staff members and participants to negotiate, although some staff members commented that it had grown easier over time.[17] One reason may be that while program staff members gave considerable emphasis to putting the Phase I components in place, at many sites they appeared to give less attention to the Phase II components until participants were almost ready to enter them.[18]

Once enrollees were engaged in Phase II activities, New Chance staff were responsible for monitoring their attendance (except for those in college) and their satisfaction with these activities. Although program guidelines called for biweekly contacts with Phase II participants, such contacts tended to occur less frequently, especially if the young women moved into off-site activities. As caseloads rose, case managers were often occupied with addressing the pressing issues of the new group of enrollees in Phase I, whom they saw daily, and had little time left over for systematic check-ups on young women they did not see. New Chance staff were also responsible for contacting training program staff and work internship employers to assess enrollees' progress in these components, an activity that was time-consuming and sometimes difficult.

At a number of sites, participants in Phase II activities sometimes also attended parenting classes or social activities at the New Chance site. They also received the support services—such as child care and transportation assistance—accorded to Phase I enrollees.

Descriptions of the specific Phase II components follow.

1. **Occupational Skills Training.** Half the New Chance sites offered on-site skills

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[17] The only staff members who reported that the transition was *not* difficult were at sites offering on-site training.

[18] Data collected for the cost analysis indicate that during the period before the majority of program enrollees would be ready to enter Phase II, sites generally spent little on activities that would indicate planning ahead, such as finding work internships or skills training slots, helping enrollees get into college, or counseling participants about the next steps.

training.[19] Only in Portland, however, was this the sole training resource utilized, and the majority of young women who engaged in skills training at the other locations went off-site to do so. There were advantages and disadvantages to on-site training. On the plus side, on-site training participants were able to remain in an environment in which they already felt comfortable and to see their case managers more frequently. On the minus side, however, most sites offered training only in limited areas. (For example, the Portland site provided training only in business skills.)

In general, participants were interested in a relatively narrow range of "pink-collar" occupations; typically they sought training in clerical areas (as data processors, business machine operators, and the like) and in medical fields (as medical technicians or, frequently, as certified nurse's aides).[20] The decision about which training provider would be most suitable rested on several factors: location, entry requirements (some facilities required enrollees to have a GED upon entry, whereas others allowed them to work toward a GED concurrently with their training), slot availability, whether training was free to the participant (New Chance staff counseled participants not to enroll in programs that required them to take out loans), and the facility's general reputation and placement record (as well as its record with previous enrollees from New Chance). Training was typically financed under the JOBS program, by funding under the Job Training Partnership Act (JTPA), or (when offered through community colleges) by Pell Grants.[21]

New Chance sites differed in the degree of difficulty they experienced in making appropriate training placements. The Chicago Heights staff, for instance, found not only that it was hard to obtain slots for young women who did not have a GED but also that the majority of programs were in locations that were hard for program participants to get to.

2. **Work Internships.** Work internships were designed to provide participants with exposure to the practices and routines of work settings in general, as well as to the tasks and working conditions associated with specific kinds of jobs. Sites differed in the extent to which they made use of work internships and in their judgments as to which young women were appropriate for this service. Detroit, for example, tended to use work internships for participants with relatively low skills, while the Minneapolis staff placed in these positions only young women with a record of good attendance in Phase I, reasoning that otherwise the sponsor agency's reputation with employers would be jeopardized. Furthermore, while some sites relied on existing pools of jobs for work experience positions, others also developed individualized work internship positions for young women with specialized interests.

Work internships varied in intensity and duration. At one site, for example, they were scheduled for four hours a week for six months, while at another they occupied 15 to 20 hours a week and lasted between two weeks and three or four months. At some sites, participants worked for the experience alone; at others, they were paid (in Denver, for example, the pay was between $4.50 and $6.00 an hour).[22]

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[19] These eight sites were Allentown, the Bronx, Chula Vista, Denver, Harlem, Inglewood (for only a brief period during the demonstration), Portland, and San Jose.

[20] While the New Chance sites did not make a concerted effort to interest the young women in nontraditional occupations, it is also true that only a handful of young women expressed interest in such careers.

[21] Pell Grants are federal grants-in-aid given to economically disadvantaged college students.

[22] Where internships were paid, an agreement was usually reached with the welfare agency whereby the stipends

(...continued)

3. Job Placement Assistance. Responsibility for job placement assistance was lodged in different parties at the different sites. At some sites, it was primarily the province of the off-site training programs to which the young women were referred. At other sites, New Chance or other sponsor agency staff took on this function, and at still other sites, participants were responsible for developing their own job leads, with the assistance and encouragement of program staff. Finally, at a number of sites, responsibility for placement depended on the status of the young woman being placed (for example, whether she had received a GED or not).

4. College. While not part of the formal program model, college attendance was a post-GED activity for some of the young women. Those who attended college generally enrolled in two-year community colleges, where they took a mix of liberal arts (for example, English and psychology) and vocationally oriented courses.

The New Chance sites differed in their policies about which young women should be encouraged to attend college. Staff at some programs, reasoning that college was difficult and demanding, believed that this option should be reserved for young women who had displayed regular attendance, greater-than-average maturity, and stable living arrangements. Other sites felt that all students who wanted to attend college could benefit from this experience and that a college diploma would better enable the young mothers to attain long-term economic self-sufficiency.[23]

According to program coordinators, the young women often discovered that completing a GED was inadequate preparation for college; they were unaccustomed to lectures and note-taking, to demanding schedules, and, especially, to lengthy reading assignments. Dropping courses was common. Progress toward a diploma was also impeded by the fact that many young women were required to take remedial-level courses in English and math.

III. Participation in New Chance Activities

This section presents data drawn from the MIS on participation in program activities, first for the experimental sample as a whole and then for subgroups of the sample and for the individual sites.

A. Aggregate Measures of Participation

Table 4.2 shows several measures of participation for all New Chance enrollees (that is, all experimental group members) during the 18 months after random assignment. The table makes several points. First, as expected, a high proportion—88.5 percent—of the young women

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were not deducted from the young woman's welfare check. At one site, however, participants could not accept paid internships because they would then have been ineligible to receive child care.

[23] Some sites changed their assessments over time. The Lexington program coordinator, for example, initially believed that a college degree would give young women an important advantage in the area's generally low-wage economy. The high rate at which New Chance students dropped out of the local community college caused her to reassess this position and to see training programs as a more suitable option for many enrollees.

Table 4.2

Participation Rates, Hours of Participation, and Months of Activity for New Chance Experimentals Within 18 Months After Random Assignment

Activity Measure	Experimentals
Participated in (%)	
Any activity[a]	88.5
Phase I activities	
Adult education (ABE/GED)	85.4
Employability development	78.8
Family planning	72.2
Health education	72.3
Parenting education	79.0
Life skills	80.2
Other group activities	79.1
Phase II activities	
Skills training	32.8
Work internship	20.6
Average hours of participation in	
All counted activities[a]	296.3
Phase I activities	
Adult education (ABE/GED)	100.9
Employability development	26.3
Family planning	6.3
Health education	10.6
Parenting education	17.6
Life skills	20.4
Other group activities	19.3
Phase II activities	
Skills training	67.0
Work internship	27.8
Percentage distribution of hours in all activities	
0	11.5
1-100	24.6
101-300	25.5
301-500	16.3
501 or more	22.1
Total	100.0
Months of activity[b]	
Average	6.4
Median	5.0
Still participating in the specified month after random assignment[c] (%)	
Month 3	75.4
Month 6	58.6
Month 9	43.6
Month 12	32.1
Month 15	21.6
Month 18	11.3
Sample size	1,401

(continued)

Table 4.2 (continued)

SOURCE: MDRC calculations from New Chance MIS data.

NOTES: Calculations for this table used data for 1,401 experimentals for whom there were 42 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

[a] Excludes individual counseling and college classes.

[b] Months in which experimentals took part in New Chance activities may not have been continuous.

[c] Includes women who had dropped out but subsequently rejoined the program.

participated in some program activity.[24] Participation was less than 100 percent because of attrition during the sometimes lengthy waiting periods between random assignment and the actual start of program activities.[25]

Second, while a large majority of the young women received each of the Phase I services, a much smaller proportion took part in Phase II activities. Eighty-five percent attended ABE/GED classes, and between 72 and 80 percent of the young women took part in employability development, family planning, health education, parenting instruction, and life skills workshops. Only about a third (32.8 percent), however, participated in skills training; 20.6 percent participated in work internships, and 12.5 percent attended college (not shown in the table). The majority of GED holders (63 percent) did go on to skills training or a work internship.[26] Over two thirds (67.6 percent) of the non-GED earners, who made up the majority of participants, did not go on to Phase II components at all, and in fact many dropped out of New Chance early.

A third finding to emerge from the table is that, on average, the young women participated for just under 300 (296.3) hours in counted activities. About a third of these hours were spent in education. Although only a third of the young women participated in skills training, those who did so were in this activity for several hours a day, bringing the overall average to 67 hours.[27] Health education and personal development activities (for example, parenting education and life skills instruction) generally occupied between 10 and 20 hours each.

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[24] A young woman was considered to have participated in New Chance if she attended one of the activities shown in the table for at least one hour. Individual counseling sessions were not recorded on the MIS. It is likely that some young women who did not take part in other program activities received some counseling; thus, a "true" measure of program participation would be somewhat higher than the 88.5 percent figure shown in the table. Data on utilization of child care were also not collected on the MIS.

[25] A comparison of the characteristics of those who participated and those who did not indicates that the groups were similar in most respects. Where there was a statistically significant difference, however, it tended to suggest that the nonparticipants were more disadvantaged; they had lower educational aspirations and had been out of school longer than the participants. As a group, nonparticipants were younger; they were also less likely to have fathers who were employed, to be using birth control, or to have a child support order, but more likely to have married.

A question on the 18-month survey asked young women why they had never attended New Chance. Lack of child care (or lack of child care early enough on), pregnancy, and having moved were the three reasons most frequently cited; together they accounted for about 37 percent of the reasons given for nonparticipation. Other factors mentioned with some frequency were transportation difficulties, family problems, preference for another program, and unwillingness to attend the classes.

[26] Not all of the young women who completed a GED and were slated to move on to Phase II were interested or felt ready to do so. Program coordinators and other key personnel at some sites commented that many young mothers had enrolled in New Chance with one goal—earning a GED. Having attained it, they were much less interested in or ready to make a commitment to the program's employment objectives. Other young women felt that they needed and deserved "time off" between receiving their GEDs (an arduous process for some) and moving on to other demanding activities. Still others were anxious about encountering new experiences and about leaving the supportive environment of New Chance or had unrealistic expectations about the kinds of jobs they would be able to get with a GED and felt that they did not need additional training. One program coordinator commented that staff should have stressed the importance of the Phase II activities earlier in the participants' program stay.

[27] Another way of thinking about this statistic is that individuals who participated in skills training did so for about 200 hours, or eight weeks for programs occupying 25 hours a week.

Fourth, this 296.3 average does not reflect the wide variation in the distribution of hours of program activities attended. Along with the 11.5 percent of all experimentals who did not participate at all, another 24.6 percent participated for 100 hours or fewer. At the other end of the spectrum, 22.1 percent registered more than 500 hours. Low participation hours reflect not only early departures from the program but also frequent absenteeism, a serious problem in New Chance as in many other programs serving disadvantaged youth.[28] MDRC operations staff noted that at many sites only about half the young women enrolled in the program attended on any given day.

Fifth, enrollees were active in the program for 6.4 months on average. These months were not necessarily continuous, however, so that "months of activity" is not synonymous with "length of stay." Periods of program activity were sometimes interspersed with periods in an inactive status; thus, if a woman was active for three months, inactive for two months, and then active for another three months, she would be considered to have been active for six months but as having had a length of stay of eight months.[29] Whatever the metric, however, the average length of stay was considerably shorter than the maximum term specified in program regulations.

About a third of the young women were participating in the program a year after they were randomly assigned, and just over a tenth were still active at the time of the 18-month follow-up interview. (These figures include women who had dropped out but subsequently rejoined the program.)

Taken together, these findings indicate that New Chance was a considerably less intensive program than its planners had anticipated. This conclusion echoes data presented in the 1991 New Chance implementation report, which indicated that, owing to absenteeism and early terminations, participants generally got between 30 and 40 percent of the amount of Phase I services they could have received. (Since the length and intensity of Phase II components varied so much from one individual to another, it is not possible to derive a similar percentage for enrollees in that phase of the program.)

B. Subgroup Variation in Participation

The aggregate figures presented in the preceding section conceal a good deal of variation both among young women with different baseline characteristics and among the program sites. Table 4.3 shows the average hours of participation for subgroups of young women defined by their characteristics at random assignment. The presence of one or more asterisks indicates that the difference in average participation hours for the different subgroups was *statistically significant*— that is, unlikely to have arisen simply by chance.[30] The p-value shows the specific probability that the difference between the subgroups was the result of chance; probabilities of 0.1 or less are considered to be statistically significant. Thus, on average, young women who were 16 or 17 years old participated for 290 hours, those 18 or 19 participated for 287 hours, and those 20 to 22 participated for 314 hours; the absence of an asterisk and the p-value greater than .1 (.348) both

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[28] See, for example, Auspos et al., 1989; Higgins, 1988; U.S. Department of Education, 1988.

[29] Sites varied in their use of inactive status and in how quickly they terminated nonparticipants from the enrollee roster.

[30] The concept of statistical significance is explained in detail in Chapter 2.

Table 4.3

Average Hours of Participation of New Chance Experimentals Within 18 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Average Hours of Participation		p[a]
Age (years)				0.348
16-17	279	290		
18-19	664	287		
20-22	457	314		
Ethnicity			***	0.000
Black, non-Hispanic	723	281		
Hispanic	323	369		
White or other	353	261		
Highest grade completed			**	0.023
10th or below	936	283		
11th or above	464	323		
Interval since last attended regular high school			*	0.080
More than 2 years	728	285		
2 years or less	631	315		
TABE reading test score (grade equivalent)[b]			*	0.060
Below 6th grade	296	254		
6th or 7th grade	342	317		
8th or 9th grade	369	308		
10th grade or above	391	301		
Ever employed				0.195
Yes	1,109	302		
No	292	275		
Family received AFDC when sample member was growing up			***	0.004
Always	239	257		
Sometimes	634	283		
Never	516	331		
CES-D (depression) Scale[c]			**	0.040
0-15 (not at risk)	669	303		
16-23 (at some risk)	347	318		
24-60 (at high risk)	382	262		
Sample size	1,401			

(continued)

Table 4.3 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and MIS data.

NOTES: Calculations for this table used data for 1,401 experimentals for whom there were 42 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires. The table includes 18 months of MIS follow-up data for each individual.

[a] A t-test or F-test was applied to each regression-adjusted difference between subgroup outcomes. The column labeled "p" is the statistical significance level of the difference between subgroup outcomes: That is, p is the probability that subgroup outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[c] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

indicate that the differences in average participation hours for these subgroups were not statistically significant and could have arisen by chance alone.[31]

Higher hours of participation were associated with a number of characteristics generally indicative of being more advantaged, educationally and otherwise: having attended school more recently, having completed more years of schooling, coming from a family that never received welfare, and not being at high risk of depression. (Conversely, participants who were more disadvantaged generally participated less.) Interestingly, those reading at the eighth-grade level or above attended more hours than the very poorest readers (those reading below the sixth-grade level), but even higher attendance hours were registered by those who at entry read at the sixth- or seventh-grade level—perhaps because they needed the extra instruction to be able to pass the GED test. Finally, Hispanic young women had higher participation hours than either their white or their black counterparts, even after controlling for site differences.

C. Site Variation in Participation

Table 4.4 shows how the 16 sites performed on each of six participation indicators. The table makes clear that the sites' records varied considerably on each indicator. The proportion participating in any activity, for instance, ranged from 66.3 percent in the Bronx to 100.0 percent in Denver. Similarly, the average number of hours of participation ranged from 123.4 in Chicago Heights to 476.4, again in Denver. Denver, Detroit (except for Phase II), Pittsburgh, Portland, and San Jose performed better than average on most of the indicators shown. The Bronx, Chicago Heights, Harlem, Minneapolis, and Salem did worse than average.[32]

Disparities in hours of participation are pronounced for both Phase I and Phase II components. Because sites operated Phase I in a relatively uniform fashion, providing similar amounts of program services, disparities in Phase I participation hours reflect factors other than whether services were offered or not (they were). One possible explanation for these differences is that some sites enrolled young women who were "easier to serve"—less disadvantaged or more motivated—than others. After statistical procedures were used to adjust for these baseline differences, however, differences among the sites in the average hours of participation remained highly statistically significant (not shown in tables), indicating that enrollee characteristics played a fairly modest role in explaining differences in participation. A second possibility is that differences in Phase I participation hours were driven by differences in the proportion of young women who enrolled in the program but never participated. Again, however, when hours of participation were examined only for experimental group members who were ever active in the program, there were still marked differences among the sites.

One structural feature of the sites did appear to have a sizable effect on participation; sites

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[31] The table presents comparisons based on adjusted subgroup differences. That is, the analysis does not take into account the fact that some subgroup characteristics might be systematically related to other characteristics (for example, age and number of children) that might also affect participation.

[32] In the Bronx, those assigned to the experimental group sometimes had to wait several weeks for the next cohort to begin. Attrition during these periods accounts in part for the low percentage of women ever active there (66.3 percent) and for the site's poor performance on the other indicators (all of which include both experimentals who were active in New Chance and those who never participated at all and thus had zero hours recorded on the MIS).

Table 4.4

Participation in New Chance Within 18 Months After Random Assignment, by Site

Site	Sample Size	Participated in Any Activity[a] (%)	Average Hours in Phase I Activities[b]	Average Hours in Phase II Activities[c]	Average Hours in All Activities[a]	Participated over 300 Hours (%)	Average (Mean) Months of Activity[a]
Allentown	76	93.4	189.6	79.5	269.2	35.5	6.4
Bronx	89	66.3	128.1	38.4	166.5	21.4	3.3
Chicago Heights	43	76.7	93.6	29.8	123.4	20.9	3.3
Chula Vista	90	87.8	213.7	85.5	299.3	44.4	7.2
Denver	70	100.0	309.3	167.1	476.4	65.7	7.7
Detroit	106	98.1	297.2	69.2	366.4	44.3	6.5
Harlem	85	75.3	195.1	70.4	265.6	35.3	4.9
Inglewood	86	83.7	180.0	96.4	276.4	37.2	6.5
Jacksonville	96	97.9	183.3	67.0	250.3	34.4	6.7
Lexington	89	88.8	210.7	31.4	242.1	31.5	6.0
Minneapolis	85	94.1	158.6	58.7	217.3	22.4	5.8
Philadelphia	92	91.3	211.6	76.9	288.5	30.4	7.8
Pittsburgh	110	88.2	209.6	152.9	362.5	45.5	7.1
Portland	98	94.9	158.1	291.5	449.6	56.1	8.8
Salem	90	81.1	163.2	20.6	183.8	25.6	4.9
San Jose	96	91.7	263.8	131.9	395.8	54.2	7.7
All sites	1,401	88.5	201.4	94.9	296.3	38.4	6.4

SOURCE: MDRC calculations from New Chance MIS data.

NOTES: Calculations for this table used data for 1,401 experimentals for whom there were 42 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate. The table includes 18 months of MIS follow-up data for each individual.

[a] Excludes individual counseling and college classes.

[b] Phase I hours include time spent in adult education (ABE/GED), career exploration and pre-employment skills training, Life Skills and Opportunities curriculum, health education and health care services, family planning, adult survival skills training, parenting education, and pediatric health services. They exclude time spent in individual counseling.

[c] Phase II hours include time spent in skills training and work internships. They exclude time spent in college and job placement assistance.

with on-site child care had higher Phase I participation hours (216 hours, on average) than sites without this service (174 hours, on average). Yet sites that operated five days a week had only slightly higher average hours than sites that operated just four days a week and left the fifth day for participants' appointments and staff planning (201 versus 191 hours, on average).

Perhaps the most important factor accounting for variation among the sites in the number of young women entering Phase II activities was site variation in the proportion of young women who earned a high school diploma or GED, a topic discussed in the next chapter. Further, variation in the proportion of enrollees participating in skills training or work experience is partially explained by site differences in the amount of emphasis program operators gave to these activities.

Other site-specific factors also help to explain the degree to which sites made use of Phase II components. For example, in Inglewood, staff sought to restrict training to those who had already completed their GEDs, having learned from experience that once participants entered training, they rarely completed work toward this credential. In Philadelphia, most short-term training programs were funded through the Private Industry Council and were reluctant to enroll teens during the period under study; New Chance staff members at the Philadelphia program reported that participants were unwilling to commit to longer courses available through local community colleges. Sites also differed in their use of work internships. Chicago Heights staff, for instance, did not assign anyone to this activity, reasoning that enrollees there would be unwilling to work without pay, whereas all Portland participants were supposed to hold both a two-week and a six-week internship before leaving the program. (In Portland, it will be recalled, skills training proceeded concurrently with adult education [ABE/GED] classes; at this site, the distinction between Phase I and Phase II is therefore blurred.)

D. Sites' Responses to Absenteeism

In formal and informal interviews with MDRC personnel, staff members at some sites asserted that their sites had experienced high absenteeism in part because clear requirements and expectations had not been enunciated and emphasized from the start. Over time, these sites tried to implement more stringent policies, but there is little evidence that these policies were successful: Participation rates for earlier program enrollees (those randomly assigned through September 1990) were actually higher than those for later entrants, and the data further indicate that while attendance improved at some sites with time, at other sites it got worse.

At best, it appears that forcefully articulated rules were a necessary, but not a sufficient, condition of good attendance. That is, such rules did not result in good attendance at all sites, but all sites where attendance was *not* a particular problem did have clear rules, which they made known at the outset of a young woman's stay and reinforced periodically through group discussions and other methods.

Sites tried a number of strategies to improve attendance. Case managers tried to follow up on absenteeism immediately, calling a young woman on the same day to ascertain the reason for her absence, find out whether the problem could help resolve any problems, and stress the importance of her being there. The local programs also developed a variety of rewards for good attenders including recognition ceremonies and points redeemable for items participants valued, such as children's clothing.

IV. Impacts of New Chance on Service Receipt

For New Chance to be demonstrably effective, differences in the services received by experimentals and controls must be found to exist along at least one of several dimensions. One dimension is the *amount* of services received, as measured by the proportion of research group members getting each service, the duration of service receipt, and its intensity. A second dimension is *comprehensiveness*—that is, the number of different kinds of services received. A third dimension is the *timing* of the services; experimentals may have received services earlier and thereby have gotten a head start in moving forward in their lives. (It is also possible for service receipt to be mistimed, if services are delivered at a point in recipients' lives when they cannot make good use of them.) Finally, impacts could also arise, at least in theory, if experimentals and controls received equal amounts of service but the services received by experimentals were of higher *quality* than those received by controls.

This section uses data from the 18- and 42-month surveys to address all four of these dimensions.[33] It examines two principal kinds of services: employment preparation activities and services to enhance the young women's personal development and parenting skills.[34] (Sample members' use of child care is considered in Chapter 8.) These findings are then placed in context with an examination of service receipt in New Chance and in the other programs serving young mothers that were described in Chapter 1.

A. Impacts on the Amount of Services Received

Impacts on the amount of services received are reported in two ways: as the difference between the percentages of experimental and control group members who *ever* received a given service within the 42-month follow-up period and as the difference between the groups in the *quantity* of service, as measured either by its *duration* or by the *number of times* it was received (depending on the specific service).

1. **Impacts on Amount of Participation in Employment Preparation Activities.** Impacts on the amount of employment preparation services received are discussed for the sample as a whole and, for selected services, for subgroups of the sample defined by their demographic and socioeconomic characteristics at random assignment and for the 16 individual sites.

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[33] The evaluation did not have the resources that would have been necessary to collect rigorous data on the quality of services received by members of the two groups, although, as will be discussed later in the chapter, MDRC staff members attempted to rate the quality of service offerings at the New Chance sites. Also, questions on the 18-month survey asked experimentals and controls to rate services they had received; these ratings also appear later in the chapter.

[34] The participation measures for experimentals presented in this part of the chapter, coming as they do from the 18- and 42-month surveys and based on sample members' self-reports, differ from those appearing in Section III, which were based on MIS data collected by site staff and reported to MDRC. The two data sources inevitably produce discrepancies in results, for several reasons. First, in order to obtain comparable data for experimentals and controls, the survey captured *all* participation in various activities, while the MIS data were limited to activities directly related to the New Chance program. Also, certain New Chance activities that were reported separately in the MIS data (for example, GED preparation and life skills classes) appear to have been reported as a single activity—education—by some survey respondents; this discrepancy inflates the apparent intensity of ABE/GED services and reduces the intensity of the personal development services.

(a) Aggregate Impacts. As Table 4.5 shows, experimentals were more likely than controls (94.5 percent versus 85.9 percent) to have attended an activity designed to prepare them for employment—that is, ABE/GED or college classes, skills training, or job club[35]—during the 42-month follow-up period. This difference, however, while statistically significant, was not large, and controls received many more services than program planners had anticipated.[36]

The difference in participation rates between the two groups was, predictably, largest during the first six months after random assignment (42.4 percentage points), when most New Chance enrollees attended ABE or GED preparation classes, and narrowed rapidly thereafter. During the last two years of the follow-up period (by which time most experimentals had left New Chance), statistically indistinguishable proportions of experimentals and controls participated in these employment preparation activities, and at the time of the 42-month interview, virtually identical percentages of experimentals and controls (16-17 percent) were engaged in these components.

The same general pattern of relatively small experimental/control differences, with participation rates for the two groups converging over time, holds for most of the specific activities included under the rubric of employment preparation services. A large majority of those who participated in such activities attended ABE/GED classes: 83.5 percent of experimentals and 63.2 percent of controls.[37] Attendance in high school classes is not shown in the table;[38] the findings of the 18-month report indicate that only a handful of young women in either group chose to enroll in regular high school programs (2.5 percent of experimentals and 3.6 percent of controls). The preference for GED rather than high school programs among both groups is not surprising, given the young mothers' ages and the length of time they had been out of school (more than two years, on average), as well as the New Chance program's emphasis on GED attainment.

College attendance was not, as has been noted, a formal part of the program model. Nonetheless, over the 42-month period almost one in four experimentals (22.9 percent) and one in five controls (19.6 percent) attended college—mainly two-year institutions in which sample members pursued vocational courses of study.[39] The statistically significant although modest difference in participation rates between the two groups over the follow-up period is entirely Table

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[35] Job club consists of classes lasting a few weeks that include time spent first in learning how to prepare resumes and complete job applications and then in actually calling employers. In some cases, these classes were delivered by the New Chance program operator; in others, they were run by the local welfare department.

[36] One possible explanation for relatively high rates of service receipt by controls is that they were referred to these activities by local JOBS program or welfare office staff members. The data suggest that this was not the case, however; the proportions of experimentals and controls in each activity who reported having been referred by JOBS or welfare staff were not significantly different.

[37] Although experimentals were significantly more likely than controls to attend ABE/GED classes during the first 18 months of follow-up—presumably because they were enrolled in New Chance during this period—controls were actually more likely to attend such classes during the next two years. At the 42-month follow-up interviews, a significantly higher proportion of controls than experimentals (8.1 versus 5.6 percent) also reported they were then attending ABE/GED classes, in part because more experimentals had already earned a GED.

[38] The 18-month survey interview asked about high school attendance; the 42-month survey, however, did not ask specifically about this activity and instead included high school under the general category of "other education programs." Thus, it is not possible to examine high school attendance patterns during the last two years of follow-up.

[39] Thus, the distinction between college and skills training often referred more to the type of institution providing the training and to the duration of the course of study (with college taking longer to complete) than to the content of the classes.

Table 4.5

Impacts of New Chance on Participation in Employment Preparation Activities at or Within 42 Months After Random Assignment

Activity and Follow-Up Period	Participated in Activity				Average Number of Weeks Participated			
	Experimentals (%)	Controls (%)	Difference	p[a]	Experimentals	Controls	Difference	p[a]
Any education, skills training, or job club								
Months 1-6	82.1	39.7	42.4 ***	0.000	16.6	5.4	11.2 ***	0.000
Months 7-18	64.2	58.5	10.7 ***	0.000	19.5	13.6	5.9 ***	0.000
Months 19-30	45.2	46.2	-1.0	0.656	8.6	8.9	-0.3	0.663
Months 31-42	41.4	43.2	-1.8	0.427	8.4	8.5	-0.2	0.780
Months 1-42	94.5	85.9	8.6 ***	0.000	53.1	36.5	16.6 ***	0.000
Attending any education, skills training, or job club at 42-month follow-up	15.9	16.8	-0.9	0.590				
Adult education (ABE/GED)								
Months 1-6	73.0	25.8	47.3 ***	0.000	11.5	2.9	8.6 ***	0.000
Months 7-18	46.3	34.6	11.7 ***	0.000	9.2	5.8	3.4 ***	0.000
Months 19-30	20.3	24.6	-4.3 **	0.025	2.8	3.9	-1.1 ***	0.006
Months 31-42	18.3	23.5	-5.2 ***	0.005	2.7	3.7	-1.0 ***	0.010
Months 1-42	83.5	63.2	20.3 ***	0.000	26.2	16.3	9.9 ***	0.000
Attending adult education (ABE/GED) at 42-month follow-up	5.6	8.1	-2.5 **	0.028				
College								
Months 1-6	3.7	3.0	0.6	0.455	0.4	0.4	0.0	0.781
Months 7-18	12.0	8.2	3.8 ***	0.006	2.8	2.0	0.8 *	0.054
Months 19-30	12.6	11.7	0.9	0.572	3.0	2.6	0.4	0.365
Months 31-42	13.0	11.0	2.0	0.188	3.2	2.7	0.5	0.306
Months 1-42	22.9	19.6	3.3 *	0.078	9.4	7.7	1.7	0.133
Attending college at 42-month follow-up	5.8	5.5	0.3	0.776				

(continued)

Table 4.5 (continued)

Activity and Follow-Up Period	Participated in Activity				Average Number of Weeks Participated			
	Experimentals (%)	Controls (%)	Difference	p^a	Experimentals	Controls	Difference	p^a
Any education program								
Months 1-6	76.8	33.4	43.4 ***	0.000	12.4	4.2	8.2 ***	0.000
Months 7-18	57.8	48.8	9.0 ***	0.000	13.1	9.8	3.3 ***	0.000
Months 19-30	32.7	35.8	-3.0	0.171	5.8	6.3	-0.5	0.355
Months 31-42	29.7	32.2	-2.6	0.232	5.8	6.2	-0.4	0.511
Months 1-42	89.2	76.2	12.9 ***	0.000	37.1	26.4	10.6 ***	0.000
Attending any education program at 42-month follow-up	11.2	12.9	-1.7	0.267				
Skills training								
Months 1-6	16.3	9.5	6.8 ***	0.000	2.6	1.1	1.5 ***	0.000
Months 7-18	28.4	18.8	9.6 ***	0.000	5.0	3.9	1.1 **	0.031
Months 19-30	19.4	16.8	2.7	0.140	3.0	2.5	0.5	0.158
Months 31-42	17.4	15.3	2.1	0.222	2.8	2.6	0.3	0.498
Months 1-42	47.5	38.1	9.4 ***	0.000	13.4	10.1	3.3 ***	0.001
Attending skills training at 42-month follow-up	5.4	4.8	0.7	0.530				
Job club								
Months 1-6	19.3	5.5	13.8 ***	0.000	3.0	0.4	2.6 ***	0.000
Months 7-18	18.6	10.8	7.8 ***	0.000	2.9	1.2	1.8 ***	0.000
Months 19-30	6.2	5.1	1.0	0.354	0.5	0.5	0.0	0.857
Months 31-42	5.4	3.6	1.8 *	0.075	0.4	0.3	0.1	0.595
Months 1-42	33.1	18.4	14.7 ***	0.000	6.8	2.5	4.4 ***	0.000
Attending job club at 42-month follow-up	0.9	0.4	0.4	0.287				
Sample Size	1,401	678						

(continued)

Table 4.5 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The total number of weeks across the various employment preparation components is less than the sum of the number of weeks in the various components because in calculating the total, weeks during which sample members participated in more than one component are counted only once.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

accounted for by the between-group difference in college attendance during months 7–18—presumably just after the experimentals had left New Chance.

Experimentals were also more likely than controls to participate in vocational skills training over the 42 months (47.5 percent versus 38.1 percent), but again, this difference was relatively small[40] and was explained by experimentals' higher rates of participation in training during the first 18 months of follow-up. Significantly higher participation by experimentals in job club also occurred largely during the first 18 months.

The right-hand section of the table shows amount of participation using a different metric: the average number of weeks experimentals and controls actually attended a given activity.[41] The number of weeks shown for each group averages together both sample members who never participated (and whose number of weeks in the activity was therefore zero) and those who did participate; because all sample members were included, the differences between the groups represent true program impacts. The data indicate that experimentals participated in education programs, skills training, and job club significantly longer than did controls. Further analysis (not presented in tables) shows that experimentals who participated in a given activity did so for longer periods than control group members who also participated in that activity. Thus, for example, over the 42-month period, experimentals who took part in ABE/GED programs were active in such programs for 31.5 weeks on average, while controls were active for 25.4 weeks. For skills training, the comparable figures were 28.3 weeks for experimentals who participated in the activity and 26.1 weeks for controls who did so. During the second part of the follow-up period, however, differences in the amount of participation between the two groups became much smaller and often disappeared.

(b) Impacts for Subgroups. Sample members' demographic, socioeconomic, and education-related characteristics at baseline can interact with experimental or control status to affect program impacts and their magnitude. (A hypothetical example: Within the subgroup of older sample members, experimentals might register higher service receipt than their control group counterparts, while within the subgroup of younger sample members, levels of service receipt for the two research groups might be very similar.) In considering subgroup impacts, two questions are of interest. First, is there an impact (that is, a statistically significant experimental/control difference) for a particular subgroup defined on the basis of a specific baseline characteristic (or set of characteristics)? And second, are the impacts for different subgroups within a given category significantly different from one another?

Overall, the data do not suggest a strong pattern of subgroup differences with respect to service receipt. Rather, where a substantial experimental/control difference existed for the sample as a whole, it also tended to hold up across the various subgroups. In the few instances where differences in the magnitude of the program impacts for particular subgroups were statistically significant, the group that was more disadvantaged at baseline tended to register the larger impact.

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[40] Relatively high participation by controls in skills training might occur if controls were more likely to find skills training courses that did not require a high school diploma or GED as a condition of entry. This hypothesis is not borne out by the data, however; approximately 90 percent of the members of each research group had a diploma or GED certificate in hand when they entered training.

[41] These weeks were not necessarily continuous; up to six separate periods of participation were reported.

For example, the impact on the percentage of sample members ever attending college was greater for those who had never worked than for those who did have prior work experience, and the impact on the number of weeks of attendance in education programs was greater for those who had completed tenth grade or less than for those who had completed eleventh grade or higher. (In the first case, this was because experimentals without previous employment experience were actually more likely to go to college than those who had worked before; in the second instance, it was because controls who had completed tenth grade or less attended school for many fewer weeks than those who had completed eleventh grade or more.)

It is worth noting that subgroup patterns differ with respect to *outcomes* and *impacts*. That is, when participation rates of various subgroups of experimentals were compared with each other, those subgroups who were more advantaged were found to have better outcomes. When participation rates of experimentals were compared with those of controls, however, larger impacts were found to accrue to the more disadvantaged groups. Without the assistance of New Chance, less advantaged groups were less likely to receive services; thus, experimental/control differences were larger for these groups.

Subgroup results with regard to attendance in job club appear to constitute an exception to the general finding of greater impacts for the more disadvantaged. Impacts on job club participation were higher for experimentals who did not receive AFDC and who had been employed in the year prior to random assignment. It may be that more disadvantaged experimentals and controls (that is, those receiving AFDC) were more likely to participate in job club under the auspices of the JOBS program.

(c) Impacts for Sites. The strong positive impact registered by New Chance on weeks of attendance in education activities held up at 10 of the 16 sites. Similarly, the program's effect on participation in job club was sustained across the majority of sites. In contrast, the program's positive impact on participation in skills training and college attendance appears to have been driven by statistically significant differences favoring experimentals at just a few locations (Allentown, the Bronx, and Portland for skills training; Inglewood and San Jose for college attendance).

The Allentown and Inglewood New Chance programs appear to have been especially effective in boosting experimentals' levels of participation above the levels attained by controls. The Minneapolis site, in contrast, seems not to have succeeded in this regard; there, controls were more likely than experimentals to attend ABE/GED classes.

2. Impacts on Participation in Parenting and Personal Development Activities. As Table 4.6 makes clear, experimentals were much more likely than controls to participate in parenting classes and to receive other services aimed at furthering their personal development: classes on family planning, health, and life skills, and personal and job counseling. While the large majority of controls—85.9 percent—participated in employment preparation activities of some kind during the 42-month follow-up period, as is shown in Table 4.5, a lower (but still sizable) proportion—61 percent—received any of the services that fall under the rubric of parenting or personal development services, compared with 83.9 percent of the experimentals.

Experimentals not only were more likely to participate at all in these activities but also

Table 4.6

Impacts of New Chance on Receipt of Services to Enhance Personal Development Within 42 Months After Random Assignment

Activity and Frequency of Attendance/Receipt	Months 1-18				Months 19-42				Months 1-42			
	Experimentals (%)	Controls (%)	Difference	p[a]	Experimentals (%)	Controls (%)	Difference	p[a]	Experimentals (%)	Controls (%)	Difference	p[a]
Parenting classes												
Ever attended	66.8	21.3	45.5 ***	0.000	21.4	19.2	2.2	0.273	70.1	32.7	37.4 ***	0.000
10 times or fewer	26.6	11.0	15.6 ***	0.000	12.0	11.7	0.3	0.843	--[b]	--	--	--
11 times or more	40.2	10.2	29.9 ***	0.000	9.4	7.6	1.9	0.181	--	--	--	--
Never attended	33.2	78.7	-45.5 ***	0.000	78.6	80.8	-2.2	0.273	29.9	67.3	-37.4	0.000
Family planning classes												
Ever attended	52.6	12.3	40.3 ***	0.000	12.2	10.8	1.4	0.373	54.7	20.3	34.3 ***	0.000
10 times or fewer	30.0	9.8	20.1 ***	0.000	9.8	9.5	0.3	0.816	--	--	--	--
11 times or more	22.6	2.5	20.1 ***	0.000	2.4	1.3	1.0	0.132	--	--	--	--
Never attended	47.4	87.7	-40.3 ***	0.000	87.8	89.2	-1.4	0.373	45.3	79.7	-34.4	0.000
Health classes												
Ever attended	50.0	11.1	38.9 ***	0.000	13.0	11.0	2.0	0.217	53.6	18.8	34.9 ***	0.000
10 times or fewer	25.6	8.4	17.2 ***	0.000	8.4	7.8	0.6	0.636	--	--	--	--
11 times or more	24.5	2.8	21.7 ***	0.000	4.6	3.3	1.3	0.171	--	--	--	--
Never attended	50.0	88.9	-38.9 ***	0.000	87.0	89.0	-2.0	0.217	46.4	81.2	-34.8	0.000
Personal counseling												
Ever attended	41.5	15.0	26.5 ***	0.000	16.9	11.8	5.1 ***	0.003	47.8	23.6	24.3 ***	0.000
10 times or fewer	23.5	8.7	14.8 ***	0.000	9.8	6.8	3.0 **	0.029	--	--	--	--
11 times or more	18.1	6.3	11.7 ***	0.000	7.1	5.0	2.1 *	0.078	--	--	--	--
Never attended	58.5	85.0	-26.5 ***	0.000	83.1	88.2	-5.1 ***	0.003	52.2	76.4	-24.2	0.000
Job counseling												
Ever attended	53.7	19.5	34.2 ***	0.000	19.7	16.1	3.6 *	0.054	59.2	30.9	28.3 ***	0.000
10 times or fewer	27.4	10.3	17.0 ***	0.000	12.1	10.0	2.1	0.167	--	--	--	--
11 times or more	26.3	9.2	17.1 ***	0.000	7.6	6.1	1.5	0.238	--	--	--	--
Never attended	46.3	80.5	-34.2 ***	0.000	80.3	83.9	-3.6 *	0.054	40.8	69.1	-28.3	0.000

(continued)

Table 4.6 (continued)

Activity and Frequency of Attendance/Receipt	Months 1-18				Months 19-42				Months 1-42			
	Experimentals (%)	Controls (%)	Difference	p[a]	Experimentals (%)	Controls (%)	Difference	p[a]	Experimentals (%)	Controls (%)	Difference	p[a]
Life skills classes												
Ever attended	52.0	12.4	39.6 ***	0.000	11.8	9.3	2.5	0.105	54.9	18.3	36.6 ***	0.000
10 times or fewer	23.4	5.9	17.5 ***	0.000	6.9	6.1	0.9	0.477	--	--	--	--
11 times or more	28.6	6.5	22.0 ***	0.000	5.0	3.3	1.7 *	0.087	--	--	--	--
Never attended	48.0	87.6	-39.6 ***	0.000	88.2	90.7	-2.5	0.105	45.1	81.7	-36.6	0.000
Any personal development services	79.3	45.5	33.9 ***	0.000	42.3	38.7	3.6	0.120	83.9	61.0	22.8 ***	0.000
Sample size	1,401	678										

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] Data on the number of times attended was available only for each of the two separate survey periods, not for the 42-month follow-up period as a whole.

received all services in significantly larger amounts. For example, during the first 18 months of follow-up, four times as many experimentals as controls reported attending parenting and life skills classes 11 or more times during the follow-up period; disparities between the groups in the reported receipt of health education and family planning classes were even sharper.[42]

The increase in service receipt among experimentals was registered almost entirely during the first 18 months of the follow-up period, however, when experimentals were still in New Chance. During the last two years of follow-up, experimentals' receipt of parenting and personal development services dropped sharply and was no longer significantly different from that of controls, with two exceptions; experimentals remained more likely than controls to receive both personal counseling and job counseling. (In contrast, the proportions of controls receiving these services, and the frequency with which they received them, remained remarkably constant over both parts of the follow-up period.)

Furthermore, many experimentals received only a modest level of personal development services. While the accuracy of participants' self-reports is uncertain,[43] it is nonetheless striking that about one-third of the experimentals said that they had never attended parenting classes, and 27 percent said they had attended such classes 10 times or fewer. Over three quarters (76 percent) reported equally little participation in health classes. Since parenting and health classes generally were scheduled for about an hour and a half, another way of thinking about these statistics is that only 40 percent of the experimentals received more than 15 hours of parenting instruction, and only one quarter received 15 or more hours of instruction about health issues.

One conclusion that might be drawn from these data is that both experimentals and controls were much less interested in parenting and personal development services than they were in improving their educational status. While they were in the program, experimentals participated in the full complement of program services willingly enough, but once out of New Chance, they did not seek assistance in most of these areas any more than did the controls. Another possible explanation is that personal development services were not as widely available as ABE/GED programs, or that they were less easy for controls to gain access to.

B. Impacts on the Comprehensiveness of Service Receipt

New Chance was intended to offer a comprehensive array of services aimed at improving the economic and personal well-being of the young mothers it served and of their children. Several features of the program model—for example, on-site service delivery and case management—were designed to help ensure that enrollees would receive the full set of program services.

The data presented in the previous section suggest that controls may have been less interested in some services than in others. But if controls were looking for assistance in a variety of areas, they generally had to assemble their own service packages, going from one agency to another to get the services they wanted.

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[42] Frequency of receipt was also compared only for those sample members who received the services. During the first 18 months of the follow-up period, experimentals who received parenting and personal development services got significantly more of most of them than did their control counterparts. During the second part of the follow-up period, however, experimentals and controls who received these services got them in similar amounts.

[43] As has been noted, there is reason to believe that experimentals did not always distinguish between time spent in personal development classes and workshops and time spent in ABE/GED classes.

A reasonable hypothesis, then, is that experimentals would receive a more comprehensive treatment—that is, a larger number of different services—than controls. The data confirm that this was, in fact, the case. Table 4.7 shows the distribution of experimentals and controls by the number of different services they received during the 42-month follow-up period. (The maximum number of services sample members could receive was 11.[44]) Nearly half the controls (48.5 percent), compared with 22.0 percent of the experimentals, received two services or fewer. At the other end of the spectrum, 28.6 percent of the experimentals, but only 4.0 percent of the controls, received eight services or more. The average number of services received by experimentals was 5.3; the average for controls was only 3.0.

Comprehensiveness, it appears, needs to be understood in both relative and absolute terms. Although experimentals received considerably more services than did controls, it is also clear that many experimentals did not receive the full array of services that program planners intended.

C. Impacts on the Timing of Service Receipt

The data presented in Tables 4.5 and 4.6 suggest that experimentals received more services than controls, although the difference with respect to employment preparation services was not as large as had been envisioned and the absolute level of receipt of personal development services was often low. Experimentals also received these services sooner—that is, it took controls longer to find the services they used. Additional analyses were performed to provide more precise estimates of the extent to which New Chance accelerated entry into ABE/GED classes and vocational skills training.[45]

One set of estimates uses data for the entire sample and can therefore be considered to be the estimate of a true impact.[46] The analysis indicates that New Chance accelerated entry into ABE/GED classes by 17 months and into either education or training by 10 months. Another set of estimates examines the speed of entry into these activities only for those who actually participated in these activities (so that the results cannot be considered to be program impacts). These estimates indicate that experimentals entered ABE/GED classes 9 months sooner than controls; their entry into any education or training was hastened by 8 months.

A final point about the timing of service receipt: While experimentals' use of employment preparation services declined markedly over the follow-up period, this drop in use should not obscure the fact that large proportions of both experimental and control groups took part in such activities up to three and a half years after sample entry. About 45 percent of the members of both

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[44] The services that were counted included ABE/GED classes, vocational skills training, college, other education and training (including high school), job club, parenting classes, health classes, family planning classes, personal counseling, job counseling, and life skills classes.

[45] The follow-up surveys collected information on the date that respondents first entered activities in the area of employment preparation. It did not collect comparable information on the date of first entry into services related to parenting and personal development.

[46] The underlying statistical procedure, a Tobit analysis, rests on the assumption that all experimentals and controls will eventually receive the service in question, although not within the confines of the follow-up period.

Table 4.7

Percentage Distribution of New Chance Sample Members, by Number of Services Received Within 42 Months After Random Assignment

Number of services, months 1-42	Experimentals (%)	Controls (%)
Distribution of service receipt		
0	2.8	9.1
1	9.4	19.5
2	9.8	19.9
3	8.2	17.4
4	8.9	11.8
5	8.4	8.6
6	10.4	4.6
7	13.6	5.2
8	14.9	2.5
9	10.4	1.5
10	3.3	0.0
11	0.0	0.0
	100.0	100.0
Average number of services	5.3	3.0
Sample size	1,400	678

SOURCE: Calculations from New Chance survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

A chi-square statistic indicates that the difference between the two distributions was statistically significant.

The columns actually add to 100.1 because of rounding.

groups used these services at some point during months 19–30, and about 40 percent during months 31–42. Such participation might well be expected to slow down entry into the labor market.

D. Service Quality: Sample Members' Assessments of the Services They Received

The evaluation included several attempts to measure service quality, some more systematic than others. As was previously noted, MDRC staff members conducted systematic ratings of the quality of on-site child care for the children of New Chance participants. Each MDRC staff member also rated the services at the sites to which she or he was a liaison, evaluating both their overall quality and their fidelity to the guidelines. Finally, the follow-up interviews asked sample members to rate the services they had received.

The last set of ratings provides insight into the young mothers' degree of satisfaction with these services and the extent to which they believed the services to be worthwhile and to respond to their needs. Participants' views of the program could also, in theory, help to explain the high degree of absenteeism many sites experienced.

On the 18-month survey, both experimentals and controls who received a specific service were asked to indicate how much they would recommend that service to a friend, using a scale of 0 to 10 (with 10 being the highest possible recommendation). Table 4.8 shows the survey respondents' average ratings of 12 different services. The ratings are descriptive only; impacts cannot be derived from them, because only those sample members who got the service were asked the question.

The table reveals that members of both groups tended to recommend the services they received quite favorably. All but one service received a rating of 7 or higher, and the ratings for the different kinds of services were similar. (The exception was a rating of 5.6 given to high school classes by the very small number of controls who attended them.) Experimentals' ratings of services tended to be higher than those of controls, but rarely by as much as half a point, suggesting that experimentals did not receive substantially higher-quality services than did controls.

As part of the 18-month follow-up survey, half the New Chance enrollees, chosen at random, were also queried about several aspects of their program experience. They were asked to rate a number of program dimensions using an 11-point scale, where 0 meant "not at all" and 10 "the most possible." Table 4.9 shows the averages across all sites in response to seven questions. Ratings for all dimensions were on the positive side, and enrollees were especially likely to feel that New Chance staff members cared about them as people. The young women tended to be more ambivalent, however, about whether they had been expected to spend too much time at the program; the average of the responses to this question was nearer the theoretical midpoint of the scale (5) than was the case with any other question.[47]

The young women were also asked what they had most liked and disliked about the

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[47] It is also possible that the wording of this question was confusing to some young women. Whereas more favorable assessments of the program on the other dimensions were associated with *higher* scores on the 0 to 10 scale, in this case, a young woman who felt she was *not* required to spend too much time at the program needed to indicate this with a *lower* rating.

Table 4.8

New Chance Sample Members' Ratings of Various Activities in Which They Participated, at 18 Months After Random Assignment

	Experimentals		Controls	
Activity	Sample Size	Average Rating	Sample Size	Average Rating
High school	16	8.2	8	5.6
Adult education (ABE/GED)	539	7.9	147	7.8
College classes	92	8.1	23	8.3
Other education classes	54	7.8	34	7.8
Skills training	237	8.0	72	7.0
Job club	205	8.2	39	8.0
Parenting classes	872	8.3	133	8.2
Family planning classes	686	8.7	78	8.1
Health classes	653	8.4	70	7.7
Personal counseling	542	8.4	93	8.4
Job counseling	701	8.4	122	8.0
Life skills classes	680	8.5	79	8.6

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Sample members were asked if they had attended the activity and, if so, how much they would recommend that a friend also attend it. Ratings were on an 11-point scale, where 0 meant "not at all" and 10 meant "the most possible."

This table contains data only for those who participated in a given activity rather than for the full research sample. Sample sizes vary because varying numbers of sample members participated in different classes or activities.

For controls, services were obtained at or arranged through programs or agencies other than New Chance. For experimentals, the services were obtained at or arranged through New Chance or, if they were served by additional programs, by these programs.

Table 4.9

Experimentals' Ratings of New Chance Program Features and Sites at 18 Months After Random Assignment

Program Feature or Site	Average (Mean) Rating
General program features[a]	
How much do you think the staff cared about you as a person?	8.0
How much did your case manager help you to get services or other things when you needed them?	7.7
How much did the program help you to achieve your personal goals?	6.8
How much do you feel that you were expected to spend too much time at the program?	4.3
How much did you learn in the adult education classes?	7.5
How much did you learn in the employment-related activities?	7.2
How much did the program help you with being a parent?	7.0
Average for seven program features, by site[b]	
Allentown	7.4
Bronx	6.9
Chicago Heights	7.9
Chula Vista	7.4
Denver	8.1
Detroit	6.1
Harlem	7.1
Inglewood	5.8
Jacksonville[c]	7.7
Lexington	6.8
Minneapolis	7.4
Philadelphia	7.2
Pittsburgh	7.4
Portland	6.7
Salem	7.0
San Jose	8.0
All sites	7.1
Sample size	543

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 543 randomly selected experimentals for whom there were 42 months of follow-up survey data and who were asked the questions.

[a] Ratings were on an 11-point scale, where 0 meant "not at all" and 10 meant "the most possible."

[b] In calculating this mean, the average score for "How much do you feel that you were expected to spend too much time at the program?" was inverted to achieve consistency with the other scores, where a higher number indicated a more positive rating.

program. They reported especially liking the staff, the caring, support, and individual attention they received, the other students, and the opportunity to meet new people. Over 30 percent of the respondents could think of nothing they disliked about New Chance. Interestingly, however, while most enrollees reported liking their fellow students, the single most disliked aspect of the program, cited by about one in 12 respondents, was, again, the other students (or at least some of them).

It is reasonable to hypothesize that differences in participation rates may reflect differences in the relative strength or weakness of program services and staff at the different sites and how well they fit with enrollees' needs. Yet there appears to be only a weak relationship, if any, between a site's ranking in participants' ratings and its overall rate of participation. Participants were also asked to rate the sites across these seven dimensions (see the bottom panel of Table 4.9), and of the three sites (Chicago Heights, Denver, and San Jose) rated highest by participants, participation was unusually high at only two (Denver and San Jose); it was unusually low in Chicago Heights. Moreover, the two sites that received the lowest ratings (Detroit and Inglewood) did not register especially low participation; in fact, the Inglewood program was especially successful in raising experimentals' levels of service utilization above those of controls. By the time of the 42-month follow-up, it had been almost two years (23 months), on average, since the New Chance participants had last had contact with the program. Asked in retrospect to rate their degree of satisfaction with the program on a scale of 0 (completely dissatisfied) to 10 (completely satisfied), 41.6 percent gave the program the highest rating, and 64.7 percent gave it a rating of 8, 9, or 10. Fewer than half (46.6 percent), however, recalled someone on the program staff whom they admired and hoped they could be like. (Where such a role model existed, it was often the respondent's case manager; program directors and ABE/GED teachers were also cited with some frequency.) Moreover, while just over a fifth of the respondents (21.4 percent) reported that they kept in touch with two or more of their fellow participants, 62.4 percent said that they had not kept up contact with any of their counterparts in the program. Thus, New Chance did not materially change the peer groups of most enrollees.

Finally, asked what effect New Chance had had on their lives, 35.8 percent of the respondents reported that that effect was moderately positive, and 48.3 percent said that it was somewhat positive. Less than 3 percent described the effect as somewhat or very negative. And 13 percent opined that New Chance had had no effect—either positive or negative—at all.[48]

V. The Costs of New Chance

A. Introduction

This section presents estimates of the costs of implementing the New Chance program and providing program services. The discussion distinguishes among several different types of program costs, summarized in Figure 4.1, each of which is useful in answering a particular set of questions. Boxes 1 and 2 of the figure show the costs directly associated with the operation of the New Chance program for sample members in the experimental group. The first box shows costs

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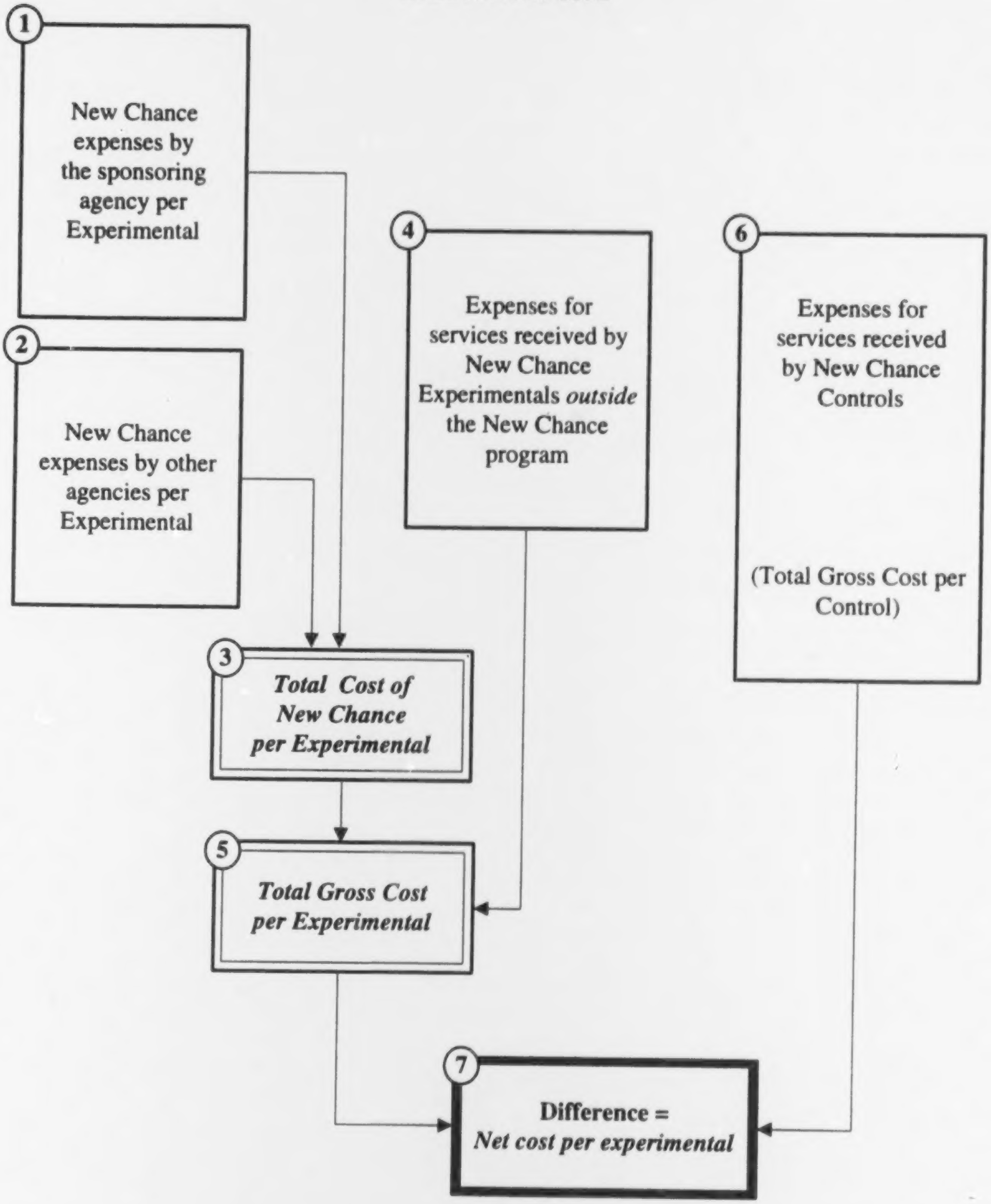
[48] About one in six experimentals who said that New Chance had not had any effect on their lives had never participated in the program; another 52 percent had participated for 129 hours or less. In contrast, 46 percent of those who believed that New Chance had had a very positive effect participated in the program for 387 hours or more.

Figure 4.1

Simplified Depiction of the Major Elements of Gross and Net Costs


1
New Chance expenses by the sponsoring agency per Experimental
2
New Chance expenses by other agencies per Experimental
3
Total Cost of New Chance per Experimental
4
Expenses for services received by New Chance Experimentals *outside* the New Chance program
5
Total Gross Cost per Experimental
6
Expenses for services received by New Chance Controls
(Total Gross Cost per Control)
7
Difference =
Net cost per experimental

incurred by the New Chance sponsoring agencies, while the second box contains costs incurred by other agencies and service providers as part of their contribution to the New Chance program. Together these costs represent the New Chance program costs, or Box 3 in Figure 4.1. Knowing these costs is useful for program planners who might consider developing interventions similar to New Chance.

These program costs, however, do not capture the value of all the services provided to New Chance enrollees over the course of the 42-month follow-up period. Some sample members who were offered access to the New Chance program did not participate or dropped out prematurely and instead received other services in the community. Other sample members participated in education or training after they completed their New Chance participation. Once the costs associated with these non–New Chance services (shown in Box 4 in Figure 4.1) are added to the New Chance program costs, the total may be referred to as "gross costs," which can be expressed on a per-enrollee basis to get a measure of the total cost of services received by the average experimental group member (shown in Box 5). This measure of gross costs captures the total value of services received by enrollees during the follow-up period. It is a useful measure of the total investment (in terms of services) that was made on behalf of sample members in the experimental group.

The next step is to produce a similar "gross cost" estimate for members of the control group. While control group members did not have access to the New Chance program and sponsoring agencies did not incur program costs on their behalf, many of them did participate in alternative education and training programs in the community. In doing so, some members of the control group may also have used case management and child care services comparable to those available to New Chance participants. Gross costs per control group member are shown in Box 6. These costs answer the important question of what expenditure levels on behalf of the young women in the program would have been if no New Chance program had been available in the community. If these levels are substantial compared with the New Chance program costs, the additional investment on behalf of the New Chance experimental group is marginal. This would also make a large relative payoff in terms of program outcomes less likely. A measure of this marginal contribution of the New Chance program is shown in Box 7, which contains the net cost per enrollee. This estimate of the net cost is the number that in a benefit-cost analysis would be compared with the program benefits.

B. Sources of Cost Data

Different data sources were combined to produce the various cost estimates represented in Figure 4.1. First, New Chance program costs (Box 3 in Figure 4.1) were estimated using participation and cost data collected by MDRC researchers at each of the 16 New Chance sites. These costs were presented in Fink (1994) and in Quint et al. (1994, pp.76–84), and summarized in what follows.[49]

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[49] While these data offer a detailed and reliable measure of the expenses involved in operating the New Chance program, certain limitations need to be considered. Most important, these figures are limited to 25 months of follow-up for each New Chance experimental. That is, participation patterns and associated costs were collected for each person assigned to New Chance for a period of 25 months after that person was first assigned to the program. While most enrollees had completed their participation in New Chance by that time, a few were still enrolled, and others

(...continued)

The estimates of gross costs (for members of both experimental and control groups) were based on survey data measuring participation in ABE/GED classes and training and use of child care services. These behavioral data were combined with New Chance unit cost data or with unit cost data from other appropriate sources to produce per capita estimates of gross costs for experimentals and controls (shown in Boxes 5 and 6).

Estimates of non–New Chance costs are not as precise or reliable as the estimates of New Chance program costs. Because program data were not available for non–New Chance services, non–New Chance cost data could not be collected directly from program records, as was done to estimate New Chance program costs. Instead, non–New Chance cost estimates were produced by multiplying survey measures of sample members' activities by unit cost estimates from varying sources.[50] Therefore, these non–New Chance cost estimates are affected both by measurement error in the activity data from the survey and by error in the estimation and selection of the unit cost numbers. Potentially the most serious problem in producing these estimates is the possible lack of comparability between the activities of control group members and the activities of experimentals enrolled in New Chance. While the follow-up surveys collected comparable participation data from the two research groups, the extent to which the underlying unit costs are comparable is unclear. For example, while enrollees participated in a relatively expensive personalized ABE/GED program with small classes and much staff attention, some of their control counterparts may have spent similar amounts of time in education programs that were less expensive. In this cost analysis it was assumed that the average unit cost for these education programs was the same for experimental and control group members.

C. The Cost of Implementing the New Chance Program

Table 4.10 shows a detailed breakdown of New Chance program costs by service component and sponsoring agency. These costs include expenses for education and training activities provided through the New Chance program, as well as expenses for ancillary services, including case management and child care. With the exception of child care and case management, the cost data are broken down into two phases, which correspond with the programmatic phases introduced earlier in this chapter. The total cost of implementing New Chance (Box 3 in Figure 4.1) was $9,026 per enrollee, the bulk of which was incurred during the first phase of the program. A substantial share of the costs was accounted for by child care ($2,573 per enrollee or 28.5 percent of the total). Case management was also an expensive component, costing $2,474 per enrollee (27.4 percent of the total). Of the education and training services, 52.6 percent was paid for by the New Chance sponsoring agencies, a share that would have been smaller if participants had used more Phase II services, which were more often provided by other agencies, such as vocational schools and community colleges.

Table 4.11 and Figure 4.2 show New Chance costs by site. Total program cost per enrollee ranged from $4,758 in Chicago Heights to $16,846 in Portland, with an average of

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may have returned to the program. Program costs associated with participation beyond the 25th month after random assignment were not captured by these cost estimates. (For more technical details, refer to Fink, 1994.)

[50] Thus, for instance, the cost of skills training for controls was estimated by multiplying the average number of months controls reported receiving these services by the average cost per person-month of delivering skills training to experimentals in New Chance.

Table 4.10

Cost of New Chance per Experimental Group Member, by Component and Agency

Program Expenditure	New Chance Sponsoring Agency's Cost ($)	Other Agencies' Cost ($)	Total Cost of New Chance ($)	Percent Distribution
Phase I	4,445	690	5,135	57
Phase II	615	703	1,318	15
Total Phase I and II	5,060	1,393	6,453	71
Child care	1,078	1,495	2,573	29
Total	6,138	2,888	9,026	100
Program activities				
Adult education (ABE/GED)	960	449	1,409	16
Employability development	225	17	242	3
Health and personal development	335	88	422	5
Parenting education	235	26	261	3
Skills training	294	392	686	8
College	0	287	287	3
Total program activities	2,049	1,259	3,307	37
Recruitment and case management				
Recruitment, intake, orientation	326	0	326	4
Case management	2,365	108	2,474	27
Phase II coordination	322	24	346	4
Total recruitment and case management	3,013	132	3,146	35
Child care	1,078	1,495	2,573	29
Total	6,140	2,886	9,026	100

SOURCES: MDRC calculations from site data and MDRC fiscal, administrative, and MIS data.

NOTES: Estimates in this table used data for all 1,408 experimentals for whom participation and cost data were collected, including sample members who were randomly assigned to New Chance but did not participate.

All costs are in 1991 dollars.

Table 4.11

Cost of New Chance Components per Experimental Group Member, by Site

Site	Adult Education (ABE/GED) ($)	Program Activities					Recruitment and Case Management			Child Care ($)	Total Cost of New Chance ($)
		Employability Development ($)	Health and Personal Development ($)	Parenting Education ($)	Skills Training ($)	College ($)	Recruitment, Intake, Orientation ($)	Case Management ($)	Phase II Coordination ($)		
Allentown	2,064	133	1,473	967	210	324	118	3,384	75	2,156	10,904
Bronx	962	141	195	112	187	73	224	2,561	407	1,384	6,246
Chicago Heights	980	263	210	125	256	99	141	1,873	555	256	4,758
Chula Vista	808	64	305	110	604	121	455	2,517	523	3,034	8,541
Denver	1,426	133	550	512	889	400	160	1,605	169	1,788	7,632
Detroit	2,506	299	475	318	188	218	393	1,896	81	2,059	8,433
Harlem	1,177	210	217	64	526	179	294	3,777	194	3,197	9,835
Inglewood	1,917	602	1,063	91	346	601	915	1,827	569	2,826	10,757
Jacksonville	934	51	234	106	345	290	82	2,479	59	2,724	7,304
Lexington	2,051	185	191	128	120	441	694	1,668	524	4,458	10,460
Minneapolis	1,192	141	343	476	62	393	378	3,034	443	4,387	10,849
Philadelphia	1,238	95	78	38	524	54	235	2,402	247	1,659	6,570
Pittsburgh	1,498	430	394	109	1,336	116	291	2,864	350	2,050	9,438
Portland	1,335	611	612	754	3,413	50	277	4,114	638	5,042	16,846
Salem	1,875	331	122	88	205	165	422	1,005	128	1,458	5,799
San Jose	583	182	301	177	1,760	1,065	138	2,570	570	2,694	10,040
All sites	1,409	242	423	261	686	287	326	2,474	346	2,573	9,026

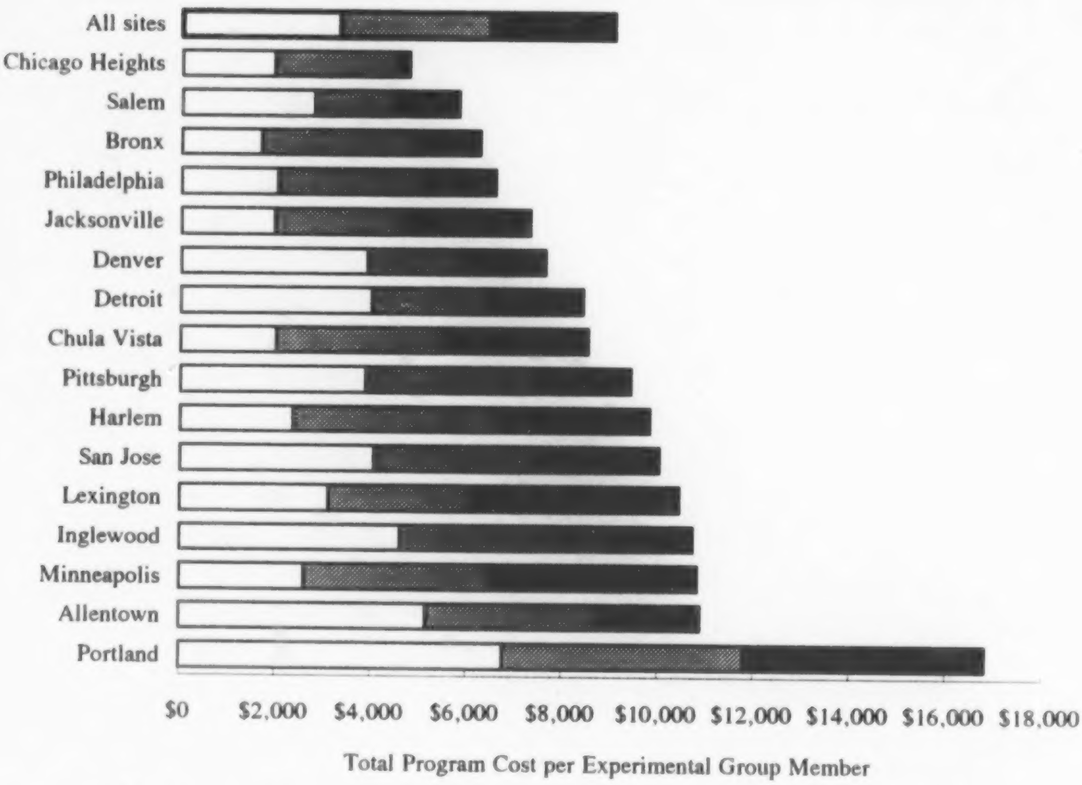
SOURCES: MDRC calculations from site data and MDRC fiscal, administrative, and MIS data.

NOTES: Estimates in this table used data for all 1,408 experimentals for whom participation and cost data were collected, including sample members who were randomly assigned to New Chance but did not participate.

All costs are in 1991 dollars.

Figure 4.2

Average New Chance Program Cost by Site



☐ Education, training, and other services ▨ Recruitment and case management ■ Child care

SOURCES: MDRC calculations from site data and MDRC fiscal, administrative, and MIS data.
NOTE: All costs are in 1991 dollars.

$9,026. As the table shows, there was a great deal of variation in the amount spent by the New Chance programs to provide the various services. Child care expenses accounted for much of the variation. They were greatest in Lexington and Portland, both of which had state-of-the-art child care centers that were dedicated primarily to the New Chance program. Child care expenses were very high in Minneapolis as well, however, even though the New Chance program in that site did not have on-site child care. (Many sample members used expensive off-site center-based child care.) Costs of the education and training services provided varied a great deal as well. Pittsburgh, Portland, and San Jose spent substantially more than any other site on skills training, while Allentown and Inglewood dedicated more resources to health and personal development classes than did the other sites. Detroit spent most of its education and training dollars on ABE/GED classes.

D. Total Cost of Services Received and Net Cost of New Chance

The next step in the cost analysis compares the cost of services received by enrollees with the cost of services received by control group members to estimate the "net cost" of the New Chance program (Box 7 in Figure 4.1). In order to make this comparison, survey data were used to estimate the extent and duration of participation in education and training by members of the control group and the use of child care by both groups. To make the comparison a fair one, the costs of service receipt by controls could not simply be compared with the costs of providing New Chance, as presented in Tables 4.10 and 4.11, because many enrollees who participated in New Chance also received services outside of the New Chance program. Instead, Table 4.12 presents comparisons of *gross* costs for experimentals and controls (Boxes 5 and 6 in Figure 4.1), which cover all services received during the 42-month follow-up period, including the use of child care while sample members were either employed or engaged in education, employment, or training services.

Table 4.12 shows two different estimates of gross and net costs, which reflect uncertainty about assumptions underlying the costs of case management and child care. For enrollees who participated in New Chance, the cost of case management that accompanied their participation was measured using cost data collected at the sites. For control group members (and enrollees) participating in alternative (non–New Chance) programs, however, no such data were collected. Consequently, it is not known to what extent those alternative education and training programs available in the community offered case management services. While many sample members who participated in education and training at schools and agencies other than New Chance probably received case management services, it is unlikely that they received as much case management as did enrollees in New Chance, where such services were specifically included in the program design. The two sets of estimates shown in Table 4.12 (labeled "Method I" and "Method II") represent two extreme assumptions about the amount of case management accompanying non–New Chance education and training. Under Method I it was assumed that sample members did not receive any case management while they participated in education and training programs outside of New Chance. On the other hand, under Method II those who participated in alternative programs were allocated a case management expense per month of service equivalent to that incurred by New Chance on behalf of the average enrollee. The net difference between these two extremes was $1,232 (resulting from increases in estimated case management costs of $1,077 for enrollees and $2,309 for control group members), reducing the

Table 4.12

Gross Cost per New Chance Sample Member and Net Cost per Experimental Group Member Within 42 Months After Random Assignment

Program Component	Gross Cost Method I (Less Case Management and Child Care) Experimentals ($)	Gross Cost Method I (Less Case Management and Child Care) Controls ($)	Gross Cost Method II (More Case Management and Child Care) Experimentals ($)	Gross Cost Method II (More Case Management and Child Care) Controls ($)	Net Cost (E-C Difference) Method I ($)	Net Cost (E-C Difference) Method II ($)
Phase I services						
Adult education (ABE/GED)	2,033	1,362	2,033	1,362	671	671
Job counseling	227	98	227	98	129	129
Health education	208	49	208	49	159	159
Family planning	155	32	155	32	123	123
Parenting	342	129	342	129	213	213
Life management skills	169	45	169	45	124	124
Subtotal phase I services	3,134	1,715	3,134	1,715	1,419	1,419
Phase II services						
Skills training	1,610	1,224	1,610	1,224	386	386
College	1,064	857	1,064	857	207	207
Subtotal phase II services	2,674	2,081	2,674	2,081	593	593
Case management and coordination						
Case management	2,474	0	3,551	2,309	2,474	1,242
Recruitment and intake	326	0	326	0	326	326
Phase II coordination	346	0	346	0	346	346
Subtotal case management and coordination	3,146	0	4,223	2,309	3,146	1,914
Child care	4,046	1,759	5,190	2,919	2,287	2,271
Total	13,000	5,555	15,221	9,024	7,445	6,197

SOURCES: MDRC calculations from site data and MDRC fiscal, administrative, and MIS data.

NOTES: Calculations for this table used data for all 2,079 sample members (1,401 experimentals and 678 controls) for whom there were 42 months of follow-up survey data, including values of zero for those who were randomly assigned to New Chance but did not participate.

All costs are in 1991 dollars.

estimated net cost of case management to $1,242 per enrollee (from $2,474). The real net expense for case management probably lies between these two numbers.

Another difference between the Method I and Method II estimates concerns the measurement of child care costs. Under Method I only the costs of center-based child care and family day care were included in the estimates, while Method II included child care provided by grandparents and other relatives. The monetary value of this type of child care was estimated from license-exempt family day care standards, which are used to determine child care allowances under the federal JOBS program.[51] Excluding these costs (Method I) results in an estimate of child care costs of $4,046 per enrollee and $1,759 per control group member. When child care by grandparents and other relatives is included, the costs increase to $5,190 per experimental and $2,919 per control. Since sample members in the two groups reported similar levels of child care by relatives, the effect on the net costs of these changes to the gross cost estimates was negligible.

When the estimates presented in Table 4.12 are taken with those presented in earlier tables, it appears that, over 42 months, New Chance enrollees received services worth about $4,000 from non–New Chance sources (Box 4 in Figure 4.1) in addition to the $9,026 worth of services they received through New Chance (using Method I, which excludes child care by relatives), for a total cost of $13,000 (Box 7 in Figure 4.1). Skills training, college, and child care accounted for most of these additional costs.

Subtracting from these gross costs per enrollee the gross costs per control (Box 6 in Figure 4.1) results in estimated net costs of $7,445 and $6,197, for Methods I and II, respectively. In dollar terms, these estimates capture the program's net contribution per sample member assigned to New Chance.

The two columns showing the net cost per enrollee (representing dollar estimates of the experimental-control difference in service receipt) demonstrate that education and training services accounted for a relatively small proportion of the program's total net contribution on behalf of those who participated. The total net cost of all Phase I services, skills training, and college combined was $2,012 (not shown in the table), or between 27 and 32 percent of the total net cost (depending on the method used to calculate case management and child care costs). Any human capital benefits of the New Chance program, including improvements in educational attainment, employment outcomes, parenting skills, health awareness, and other life skills, would have had to be produced by this arguably modest investment. The fact that the net cost of New Chance was much higher than the $2,012 spent directly on human capital services is testimony to the high cost of supporting participation in education and training by highly disadvantaged young mothers like those in New Chance. Because of this high cost, the program spent much more on supporting participation in its core services than it spent on those services themselves.

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[51] Across the 16 New Chance sites the average unit cost for this type of child care was $320 per child-month; the range was $168 to $393.

VI. New Chance and Other Programs Compared

Table 4.13 compares service receipt by experimentals and controls in New Chance and in the four other programs briefly described in Chapter 1: the JOBSTART Demonstration, the LEAP program, the Teenage Parent Demonstration, and Project Redirection.[52] The large number of cells in the table for which data are not available attests to the problems involved in drawing such comparisons, including differences in the specific variables that were examined (for example, participation in education versus participation in education or training), in the follow-up periods adopted (42 months for New Chance, 48 months for JOBSTART, 12 months for LEAP, and approximately 30 months for the Teenage Parent Demonstration), in whether the programs were mandatory (as were LEAP and the Teenage Parent Demonstration) or voluntary (as was New Chance for the most part, along with JOBSTART and Project Redirection), and in the populations they served. With regard to the last point, JOBSTART (which included youths of both sexes and young women who did and did not have children) and New Chance were targeted primarily toward high school dropouts; LEAP, the Teenage Parent Demonstration, and Project Redirection all enrolled young women who were attending school at baseline, as well as those who had dropped out; and the Teenage Parent Demonstration enrolled high school graduates as well. To give greater validity to the comparisons, the statistics for LEAP and the Teenage Parent Demonstration pertain only to the subgroup of sample members in each demonstration who were out of school and did not have a high school diploma or GED at baseline. (Even so, the populations served in these two programs were, on average, somewhat younger than the New Chance population.) The Project Redirection results are for the full enrollee population, including those who were enrolled in school at baseline, because data limited to the dropout subgroup were not consistently available.

The table indicates that, not surprisingly, substantially higher percentages of experimentals and controls participated in education and skills training in the three voluntary programs (New Chance, JOBSTART, and Project Redirection) than in the two mandatory ones (LEAP and the Teenage Parent Demonstration). This result reflects the fact that the young mothers in the voluntary programs were not typical of all young mothers who are high school dropouts; rather, they enrolled in the programs especially to receive these services.

The table also shows that controls in New Chance were more likely than controls in most of the other demonstrations to receive employment development services. Indeed, New Chance controls received more services than did the experimentals in the mandatory programs. This fact must be borne in mind when the impacts of New Chance and JOBSTART are compared as to educational attainment and employment. It means that the other demonstrations constituted a much "cleaner" test of the program model than did New Chance, where the high level of control services confounded the detection of impacts.

VII. Conclusions About Implementation

The news in this chapter is good, "bad," and mixed. The good news is that the large majority of experimentals and controls sought and received the kinds of services New Chance provided. Many of these young mothers apparently do want to better their lives and to acquire the

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[52] All these operated as demonstration programs except LEAP, which is an ongoing statewide program in Ohio.

Table 4.13

A Comparison of Program Impacts on Service Receipt in New Chance and Other Selected Programs for Young Mothers

	New Chance			JOBSTART Mothers[a]			LEAP Dropout Subsample[b]		
Service and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Controls (%)	Difference
Participated in any education program									
Quarters 1-4	80.8	48.1	32.8 ***	N/A	N/A	N/A	46.8	33.4	13.4 ***
Quarters 1-8	83.7	61.5	22.2 ***	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-16	89.2[c]	76.2[c]	12.9 ***	N/A	N/A	N/A			
Participated in any skills training program									
Quarters 1-4	24.9	14.8	10.1 ***	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	34.0	23.3	10.7 ***	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-16	47.5[c]	38.1[c]	9.4 ***	N/A	N/A	N/A			
Participated in any education or skills training program									
Quarters 1-4	86.1	54.9	31.3 ***	91.1	29.2	61.9 **	N/A	N/A	N/A
Quarters 1-8	88.7	69.4	19.3 ***	93.6	47.9	45.7 **	N/A	N/A	N/A
Quarters 1-16	94.0[c]	85.3[c]	8.7 ***	95.0[d]	59.7[d]	35.3 ***			
Received any parenting education									
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	70.1[e]	32.7[e]	37.4 ***	N/A.	N/A	N/A	N/A	N/A	N/A
Received any family planning counseling									
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	54.7[e]	20.3[e]	34.3 ***	N/A	N/A	N/A	N/A	N/A	N/A
Sample size	1,401	678		250	234		256	264	

(continued)

Table 4.13 (continued)

Service and Follow-Up Period	Teenage Parent Demonstration Dropout Subsample[f]			Project Redirection[g]		
	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Comparison Group (%)	Difference
Participated in any education program						
Quarters 1-4	N/A	N/A	N/A	75.0	51.0	24.0 ***
Quarters 1-8	42.0	21.6	20.4 **	87.0	71.0	16.0 ***
Participated in any skills training						
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	16.3	16.4	-0.1	N/A	N/A	N/A
Participated in any education or skills training program						
Quarters 1-4	N/A	N/A	N/A	N/A	N/A	N/A
Quarters 1-8	N/A	N/A	N/A	N/A	N/A	N/A
Received any parenting education						
Quarters 1-4	N/A	N/A	N/A	64.0	40.0	24.0 ***
Quarters 1-6	N/A	N/A	N/A	N/A	N/A	N/A
Received any family planning counseling						
Quarters 1-4	N/A	N/A	N/A	74.0	63.0	11.0 ***
Quarters 1-6	N/A	N/A	N/A	N/A	N/A	N/A
Sample size	1,162[h]			305	370	

(continued)

Table 4.13 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data; Bloom, 1993; Cave and Doolittle, 1991; Maynard, Nicholson, and Rangarajan, 1993; Polit and White, 1988; Quint and Riccio, 1985.

NOTES: N/A indicates that the specified data item was not available.

A two-tailed t-test was applied to regression-adjusted differences between the experimental and control groups. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The JOBSTART sample is made up of young mothers between the ages of 17 and 21 who did not have a high school diploma or GED, read below the eighth-grade level, lived with their own children, and were not enrolled in school at the time of sample enrollment.

[b] The LEAP dropout subsample is made up of teenage custodial parents (almost all of whom were women) aged 19 or younger who were on welfare, did not have a high school diploma or GED, and were not enrolled in school at the time of sample enrollment.

[c] Data are for quarters 1 through 14 only.

[d] Data from JOBSTART 48-month report. The sample sizes are 257 and 251 for experimentals and controls, respectively.

[e] Data are for quarters 1 through 6 only.

[f] The Teenage Parent Demonstration dropout subsample is made up of custodial mothers on welfare who had only one child, were between the ages of 16 and 19, did not have a high school diploma or GED, and were not enrolled in school at the time of sample enrollment.

[g] The Project Redirection sample is made up of young mothers aged 17 or younger, most of whom were on welfare. This sample includes both women who were enrolled in school and those who had dropped out. In Project Redirection, unlike other demonstrations, the comparison group was not selected through random assignment.

[h] The full sample of 1,162 is split roughly evenly between the experimental and control groups. Breakdowns by service category are not available.

skills they believe are essential to the achievement of economic well-being for their families.

The news is "bad" for those hoping that the impact results will be easily interpretable. Controls used services to a much higher degree than anticipated, and therefore the test of New Chance is not a straightforward test of extensive services compared with no services or with minimal ones. Rather, New Chance measures the effectiveness of a particular mix and level of services, delivered at a certain time, of a certain quality, and relatively easy to secure, against another mix and level of services, generally received later, of roughly similar quality, but requiring that individuals demonstrate somewhat greater initiative in order to receive them. Thus, a failure to find the expected impacts would not mean that services per se are ineffective. Indeed, as the last chapter indicates, experimentals and controls made considerable progress over time; they might have fared much worse if they had not gotten the services they did. An absence of impacts would suggest, however, that in the mix, amount, and quality of services offered, the New Chance service package was not substantially better than what was otherwise available.

The mixed news relates to the structure and substance of the New Chance program. The preceding data suggest that for a majority of enrollees, New Chance was comprehensive in that it touched on most areas of their lives and on their roles as students, prospective workers, parents, daughters, and partners. But the statistics also suggest that in many cases, because of absenteeism and early departure from the program, New Chance did little more than touch on these domains; for a sizable number of enrollees, it did not deliver services in the quantity and intensity that program planners had intended.

Data from the surveys indicate that experimentals liked New Chance. But liking the program was not enough to keep many of them actively engaged, in the face of the numerous personal difficulties many experienced. Whether they would have tried harder to attend regularly if they had been subject to a participation mandate is an open question.

The findings also suggest that employment preparation activities were the program's principal drawing card for experimentals—and for controls as well. According to staff reports, the prospect of earning a GED (coupled with the availability of free child care) was the major feature that drew young women to New Chance. Consequently, it is not surprising that of all the services New Chance offered, controls were most likely to participate in programs designed to help them earn education credentials. After experimentals left New Chance, they continued to seek these services, but at rates no higher than those of controls. Program planners saw personal development and parenting activities as intrinsic and badly needed program offerings, but the young mothers may not have agreed; in any event, they participated in them to a lesser extent. The small experimental/control differences in receipt of employment-related services, the low absolute levels of receipt of personal development services, and the degree of congruity between New Chance services and enrollees' needs as they themselves perceived them are all important factors to keep in mind in reading the chapters that follow.

Chapter 5

Impacts on Education and Training Credentials

I. Introduction

A hypothesis underlying New Chance was that participation in the program's education classes would result in better education outcomes for the young mothers and lead to their acquisition of vocational skills. Because program enrollees had typically been out of school for over two years and it would take them a long time to earn a regular high school diploma, the program model emphasized preparing students to take and pass the GED test as the principal pathway toward an education credential.[1] Raising reading scores by two grade levels was an alternative goal for those whose reading scores at program entry suggested that a GED would be unattainable within the program time frame.

Improving participants' educational status and vocational skills, perhaps the single most important short-term objective of New Chance, was also thought of as a means to an end. In line with the human capital development theory that guided the program, the acquisition of education and training credentials that enrollees could present to prospective employers was central to the program's vision of how participants could best achieve long-term well-being, economic and otherwise. "Long-term" was an important condition of the formulation, since, as will be discussed in Chapter 7, investing in education may mean forgoing immediate opportunities to work in the interest of longer-term economic gains.

This chapter first examines the impacts of New Chance on education-related outcomes: attainment of a high school diploma or GED and of college credits, along with advances in educational achievement as measured by reading test scores. It then turns to the receipt of skills training certificates.

A. The Relationship Between Educational Attainment and Other Outcomes

A large body of literature documents the strong correlation between education and positive labor market outcomes: increased rates of employment, better-quality jobs, and higher incomes (Becker, 1974; Blackburn, Bloom, and Freeman, 1989; Danziger, 1991; Levy and Michel, 1988; Mincer, 1974; Murphy and Welch, 1989). This literature holds that employers assess potential employees largely on the basis of their acquisition of a high school diploma, a college degree, or other education credentials—that is, their educational *attainment*. These credentials, in turn, are commonly accepted tokens of both educational *achievement*—the mastery of academic skills (at the most basic level, literacy and numeracy) needed to perform work tasks—and good work habits such as patience and persistence (see Berg, 1969).

Prior research also supports a strong positive association between parental educational levels and the social and cognitive development of their children (D'Amico, Haurin, and Mott, 1983;

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[1] After the period covered by this report, the Lexington program added a high school component for young women for whom earning a high school diploma appeared to be a reasonable prospect.

Desai, Michael, and Chase-Lansdale, 1990). Most of this research, however, is correlational and cross-sectional; it looks at the associations between education and other outcomes for groups of individuals at a particular point in time. Much less is known about the effects on children of increasing the educational level of their mothers, as New Chance sought to do.

Recently, the value of the GED in the labor market has become a subject of much policy debate. Some scholars have found the value of this credential to be limited, especially when compared with the labor market value of a regular high school diploma; it is argued, further, that preparation for the GED test is generally too short to add significantly to test-takers' cognitive skills.[2] Murnane and Willett (1993) express concern that the availability of the GED may induce some high school students to drop out and thereby to substitute a less valuable credential for a more valuable one. Many institutions, most notably the U.S. Army, do not accept the GED as an alternative to a regular high school diploma.

On the other hand, Murnane and Willett (1993), as well as Maloney (1991), have found that the GED appears to fulfill a "gatekeeper" function for subsequent education and training. That is, even if the credential does not appear to have much of a direct labor market impact, it is frequently a prerequisite to entry into skills training. Post-GED training, in turn, is expected to increase long-term labor market prospects for those GED attainers who use the credential to pursue it.

Also, most of the analyses informing the discussion of the value of the GED are based on the National Longitudinal Survey of Youth (NLSY), which follows a national sample of youths over time. Cave and Bos (1994) point out that individuals in this sample differ in many important ways from youths who volunteer for "second chance" education programs such as New Chance, for whom the credential may signify achievement of both a program and a personal milestone. In a nonexperimental analysis of data from the JOBSTART Demonstration, which also involved a population of high school dropouts who volunteered for remedial education and skills training, Cave and Bos found significant increases in subsequent earnings owing to program-induced GED attainment.

B. The Educational Status of the New Chance Sample at Random Assignment

Data collected at baseline indicated that, as a group, sample members faced many education-related barriers, although there was also considerable variation among individuals. While the young mothers had high educational aspirations both for themselves and for their children, the average sample member had dropped out toward the end of the tenth grade, and over a third had completed only the ninth grade or less. Reading levels for the sample as a whole varied greatly. While the sample average indicated an ability to read at the 7.6 grade level, 30 percent of sample members read at the tenth-grade level or higher, and one in seven (14.4 percent) read at the fifth-grade level or lower (see Table 3.1).

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[2] See Cameron and Heckman, 1993. Among a sample of 25-year-old men, dropouts earned $10,379, GED recipients earned $11,777, and high school graduates earned $15,214 annually. The weak earnings advantage of GED recipients over dropouts is attributed to the fact that GED recipients had an extra year of regular high school, rather than to their receipt of a GED per se.

The sponsors of the GED test themselves report that although the pass rate is about 70 percent, the average examinee spends only about 30 hours studying for the test; reportedly, many GED recipients pass it with no preparation whatsoever.

The statistics also indicated the prevalence of a number of other characteristics that might have been expected to impede the young women' s educational progress. On average, sample members had been out of school for nearly two and a half years when they entered the research sample.[3] Two in five sample members had repeated a grade, and 37.6 percent reported that they had dropped out of school before their first pregnancy, likely a sign of their alienation from school.[4]

C. A Preview of the Findings

The findings confirm that the greater participation of experimentals in education and training activities discussed in the preceding chapter resulted in significant and positive program impacts on receipt of a GED and of college credits. Given the emphasis of New Chance on GED attainment, it is not surprising that controls were slightly more likely than experimentals to earn a high school diploma. The groups did not differ in their receipt of a trade certificate or license. By the end of the follow-up period, only about one in six sample members had earned both a GED (or high school diploma) and a trade license and thus might be presumed to be especially employable.

II. Impacts on the Attainment of Education Credentials

A. Aggregate Impacts

Table 5.1 shows the impact of New Chance on receipt of several kinds of education credentials. It indicates that throughout the follow-up period, experimentals were significantly more likely than controls to have earned a GED, although the difference between the groups was not as large as program planners had expected. At the 42-month follow-up, 45.2 percent of the experimentals and 33.4 percent of the controls had received this credential.

Experimentals who earned a GED tended to do so at an earlier point than their control counterparts. Forty percent of the experimentals who obtained the credential did so within six months, and 80.8 percent within 18 months, suggesting that most earned the certificate while they were still in New Chance. Lower proportions of the controls who earned GEDs did so relatively early during the follow-up period: 28.1 percent within six months and 65.9 percent within 18 months. This finding reflects the fact that controls entered education programs later than did experimentals, as was seen in the last chapter.

As the last chapter noted, relatively few members of either group attended high school, but controls were significantly more likely than experimentals to have earned a high school diploma by the 42-month point (10.4 percent versus 6.9 percent, respectively). This finding means that the impact of New Chance on attainment of a secondary education credential (either a GED certificate or high school diploma), which was already diminished by the sizable number of controls pursuing

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[3] It is likely that many sample members attended school only sporadically before formally dropping out.

[4] The educational attainment of the enrollees' parents is also of interest. About two thirds of sample members reported that one or both parents had graduated from high school or had a GED. (Of these, 28.9 percent said that both parents were high school graduates or GED holders, while 37.4 percent knew that one parent was a high school graduate but either did not know the educational attainment of the other parent or reported that that person had not graduated.) About one third of the sample said that one or both parents had attended college.

Table 5.1

Impacts of New Chance on Receipt of Education Credentials
Within 42 Months After Random Assignment

Outcome	Experimentals (%)	Controls (%)	Difference	p[a]
Received GED by end of				
Month 6	18.6	9.4	9.2 ***	0.000
Month 18	36.5	22.0	14.5 ***	0.000
Month 30	41.4	27.6	13.8 ***	0.000
Month 42	45.2	33.4	11.8 ***	0.000
Received high school diploma by end of				
Month 6	5.3	7.2	-1.9 **	0.018
Month 18	6.4	8.9	-2.5 ***	0.008
Month 30	6.8	10.1	-3.3 ***	0.001
Month 42	6.9	10.4	-3.5 ***	0.001
Received GED or high school diploma by end of				
Month 6	23.8	16.6	7.2 ***	0.000
Month 18	42.7	30.9	11.9 ***	0.000
Month 30	48.0	37.7	10.3 ***	0.000
Month 42	51.9	43.8	8.1 ***	0.000
Received college credits by end of				
Month 18	9.6	6.6	3.1 **	0.021
Month 42	13.5	10.7	2.8 *	0.064
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

The percentages shown are for all sample members, including the 6 percent who had already achieved a high school diploma or GED when they applied to the program.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

a GED, was further attenuated by the controls' greater probability of receiving a (more valuable) high school diploma.

New Chance had a significant effect on the proportion of young women who earned college credits toward an A.A. or B.A. degree; 13.5 percent of experimentals and 10.7 percent of controls reported having earned such credits. No young women in either group had earned a four-year or a two-year college degree at the 42-month point. The proportions of sample members who actually earned college credits were several percentage points lower than the proportions who ever attended college (22.9 percent of the experimentals and 19.6 percent of the controls; see Table 4.5). Program staff at many New Chance sites noted that young women's progress toward a college degree was slowed by the need of many to take remedial classes that awarded only partial credit or no credit at all. It also appears that many young women began college but dropped out before earning any credits.[5]

B. Subgroup Impacts on Attainment of a GED or High School Diploma

New Chance had a significant and positive effect on attainment of a GED or high school diploma for most subgroups of the research sample (see Table 5.2). The magnitude of this effect was especially large, however, among young women who at baseline were more *economically* disadvantaged: those with no prior work experience and those whose families had always received welfare when they were young. (In contrast, New Chance had a negligible impact on the subgroup of young women whose families had never received welfare; over half of the controls in this subgroup earned a GED by the 42-month point.)

While program effects on educational attainment were unusually large for more *economically* disadvantaged young mothers, the program did not benefit those who were especially *educationally* disadvantaged. The table indicates that there was no impact on GED attainment for young women who entered the research sample reading below the eighth-grade level. Although it is notable that over one quarter of the young women reading below the sixth-grade level at baseline, along with some 40 percent of those reading at the sixth- or seventh-grade level, succeeded in passing the GED test, participation in New Chance did not boost rates of GED attainment significantly above the levels registered by controls.[6] At the other end of the range, the program had a significant and positive effect on GED attainment among young women with reading scores at the tenth-grade level or above; the high proportion of controls in this subgroup who received a GED is particularly notable (60.6 percent of controls versus 72.8 percent of experimentals). It appears that, in relative terms, New Chance was most successful in increasing the GED attainment of young women reading at the eighth- or ninth-grade levels at baseline, whose test-taking skills may have needed honing and whose self-confidence probably needed bolstering. Thus, the 13.8 percentage point impact on GED or high school diploma attainment registered by this group represents an increase of 30.3 percent over the control group average.

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[5] See Quint and Musick, 1994, Chapter 4, for a discussion of some of the factors that led young women to drop out of college early.

[6] Research conducted by Quint and Musick, 1994, suggests that some exceptionally low-skilled readers may have had learning disabilities that New Chance staff were not equipped to diagnose or treat.

Table 5.2

Impacts of New Chance on Attainment of a High School Diploma or GED Certificate Within 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Percentage with an Education Credential		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.272
16-17	402	50.8	44.1	6.8	0.156		
18-19	997	51.5	46.1	5.4 *	0.066		
20-22	678	52.9	40.2	12.7 ***	0.000		
Ethnicity						---	0.810
Black, non-Hispanic	1,087	52.2	43.9	8.3 ***	0.003		
Hispanic	474	50.2	40.7	9.5 **	0.028		
White or other	515	52.4	46.6	5.8	0.165		
Highest grade completed						0.3	0.944
10th or below	1,391	47.8	39.5	8.3 ***	0.001		
11th or above	684	60.2	52.2	7.9 **	0.029		
Interval since last attended regular high school						0.6	0.892
More than 2 years	1,093	49.6	41.0	8.6 ***	0.003		
2 years or less	927	54.9	46.9	8.0 **	0.010		
TABE reading test score (grade equivalent)[c]						--- **	0.026
Below 6th grade	433	25.9	28.0	-2.1	0.639		
6th or 7th grade	492	41.3	37.0	4.3	0.303		
8th or 9th grade	566	59.4	45.6	13.8 ***	0.000		
10th grade or above	583	72.8	60.6	12.2 ***	0.001		
Family received AFDC when sample member was growing up						--- **	0.021
Always	341	47.5	26.7	20.8 ***	0.000		
Sometimes	970	49.9	42.7	7.3 **	0.014		
Never	749	56.2	52.3	3.8	0.266		
Ever employed						12.1 **	0.017
Yes	1,646	52.0	46.3	5.7 **	0.013		
No	433	51.5	33.8	17.7 ***	0.000		
CES-D (depression) Scale[d]						---	0.888
0-15 (not at risk)	967	53.6	44.8	8.9 ***	0.004		
16-23 (at some risk)	525	51.3	43.1	8.2 **	0.044		
24-60 (at high risk)	582	49.6	43.1	6.5 *	0.090		
Multiple risk score[e]						---	0.840
Low	871	56.8	49.7	7.0 **	0.027		
Moderate	618	51.3	42.0	9.4 **	0.011		
High	525	45.2	35.6	9.7 **	0.019		

(continued)

Table 5.2 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as ***= 1 percent; **= 5 percent; *= 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

C. Site Impacts on Attainment of a GED or High School Diploma

At six of the New Chance sites (Allentown, the Bronx, Inglewood, Jacksonville, Pittsburgh, and San Jose), experimentals achieved positive and statistically significant gains in high school diploma or GED attainment compared with the control groups at those sites (see Appendix Table D.1). At the remaining ten sites, the difference in attainment between experimentals and controls was negligible. Overall, the variation in impacts across the sites was statistically significant.

Further analysis indicates that differences among the sites in impacts on GED attainment were no longer statistically significant once differences in the characteristics of sample members at those sites were statistically controlled for. In other words, if all sites had enrolled young women with the same initial probability of earning a GED, it is unlikely that site differences in service delivery would have produced different impacts on GED receipt.[7]

D. GED Receipt and Program Participation

As has been noted, the preceding analyses of GED impacts for the aggregate sample and for sites and subgroups all measured the impact of the *increment* in ABE/GED classes and other services that New Chance provided over and above what controls received. They did not speak to the value of education services in and of themselves (as would have been the case if controls had not received these services).

Two analyses provide some insights on the latter issue, although they do not yield definitive answers. The first analysis, descriptive in nature and confined to women in the experimental group, examines whether young women who participated more in New Chance in general, and in ABE/GED classes in particular, were also more likely to earn a GED.[8] The experimentals were divided into four groups: one group included those who did not participate at all, while the other three, of approximately equal size, divided the enrollees by their level of participation (i.e., number of hours) either in the program as a whole or in education classes. In terms of overall program participation, the proportion of women in each group who earned a GED is as follows:

	Received a GED (%)
Zero hours in New Chance	26.1
Bottom third (1 to 129 hours)	31.2
Middle third (130 to 386 hours)	51.8
Top third (more than 386 hours)	63.4

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[7] Of course, sites did *not* enroll participants who were equally likely to earn a GED. The differences in sample members' characteristics across the sites partly reflect the fact that, in addition to the program eligibility criteria that all sites used in enrolling participants, some local programs established criteria of their own (for example, excluding young women reading below a specified level because the programs felt that they could not serve them effectively). Thus, enrollees in Harlem and Minneapolis had to read at the sixth-grade level or above. San Jose imposed a 5.5-grade reading level standard initially but subsequently lowered it to fourth grade or above. Detroit, after struggling to serve many young women with very poor reading skills, eventually decided to impose a 4.5-grade reading floor.

[8] This analysis does not control for other differences among young women who participated to a greater or lesser extent in these activities.

These percentages suggest that, in terms of GED receipt, participating in New Chance only a little was not very different from not participating at all. Above that minimal level, however, there was a positive correlation between participation hours and GED attainment; the greater the number of hours, the higher the level of GED attainment.

These findings, it must be emphasized, are not impacts, and inferences must be drawn with extreme caution. It may be that if young women could have been induced to stay in New Chance longer, the program would have been more effective in helping them attain GEDs. But it is worth recalling that young women who *did* register more participation hours were generally more advantaged at baseline, educationally and otherwise, than their counterparts who were less active in the program (see Chapter 4). Furthermore, the general direction of causality in the relationship is unclear; women may have earned a GED and subsequently entered a Phase II component, so that their hours of program participation were greater for this reason.

The pattern of GED receipt in relation to hours of ABE/GED instruction was different:

	Received a GED (%)
Zero hours in ABE/GED instruction	24.8
Bottom third (1 to 47 hours)	38.1
Middle third (48 to 131 hours)	59.3
Top third (more than 131 hours)	52.3

These figures suggest that the more ABE/GED instruction received, the better the results—but only to a point, after which the law of diminishing marginal returns came into play. Instruction beyond that point yielded dividends; over half the young women who received more than 131 hours of classes were ultimately successful in earning a GED. But it took more effort on their part, and in all likelihood on the part of their teachers. It seems likely that those who were in a position to obtain a GED relatively quickly (that is, who had the requisite cognitive skills and whose other problems were manageable) did so.[9]

The second analysis used an instrumental-variables approach to develop quantitative estimates of the relationship between service intensity and receipt of a high school diploma or GED. This analysis, which used data for both experimentals and controls and counted all instruction, whether received in New Chance or not, examined credential receipt as a function of the number of weeks of ABE/GED classes attended in months 1–18 and months 1–42. The instrumental-variables technique seeks to eliminate selection bias—that is, the fact that those who received a good deal of education might be very different from those who received only a little. The analysis estimated that, after correcting for selection bias, getting no instruction at all resulted in a 40 percent rate of credential attainment. Getting some amount of instruction, but less than 18 weeks, was not statistically significantly different from getting no weeks. Getting more than 18 weeks of instruction, however, increased the proportion earning a credential to 74.3 percent. In other words, educational instruction makes a big difference in outcomes, but only if participants get a sizable amount of such instruction.

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[9] As has already been noted, these conclusions were based on comparisons of outcome levels by the amount of participation in New Chance within the experimental group and were not based on comparisons with corresponding control group outcomes. The story did not change substantially, however, when a somewhat more complex instrumental-variables approach was taken to the question of the relationship between amount of instruction and GED attainment. See Chapter 2 for details on this approach.

III. Impacts on Educational Achievement

Academic achievement was measured by administering the reading section of the Survey Form of the Tests of Adult Basic Education (TABE) to experimentals and controls as part of the 18-month follow-up.[10] This 30-item test assesses vocabulary and the ability to comprehend written materials, such as by identifying the main idea of a passage and drawing inferences.

New Chance did not have an impact on educational achievement, as measured by the TABE. Both experimentals and controls improved their reading scores between the baseline and the 18-month points to a modest but statistically significant extent: from the 7.6 to the 7.8 grade level for experimentals and from the 7.7 to the 7.9 grade level for controls. There was no significant difference between the size of the gains registered by experimentals and controls, nor between the baseline or follow-up reading levels of the two groups.

How can one explain the fact that New Chance had a substantial impact on GED attainment but not on academic achievement? Several explanations are possible. First, however, it should be noted that New Chance is not unique in this regard. Similar results—that is, impacts on GEDs but not on literacy—were found in a recent study of the implementation and effects of adult education and literacy activities in the California Greater Avenues for Independence (GAIN) Program, a welfare-to-work program for adult AFDC recipients (Martinson and Friedlander, 1994).[11]

One possible explanation for the discrepancy between impacts on GED attainment and those on literacy is that experimentals' reading ability did improve more than that of the controls but that gains had dissipated by the 18-month interview. Once having left New Chance, experimentals might not have continued to read as much as they had while they were in the program. Yet New Chance seems to have had no impact—at least as measured by the survey—on the young women' s literacy-related habits. In response to questions on the 18-month interview, experimentals and controls reported reading aloud to their children equally frequently and receiving the same number of magazines in the household.

Another possible answer is that the GED and the TABE measure different things. To earn a GED, an individual must master a specific body of information that is covered on the GED test. The GED "test" is actually a 7½-hour battery of tests that are designed to measure what graduating high school seniors in the United States are expected to know. The five component tests correspond to the general framework of high school curricula: writing skills, social studies, science, interpreting literature and the arts, and mathematics. Although the test developers assert that the ability to read, comprehend, and analyze written material is a skill needed for all five tests, there is no section that taps reading ability per se. The TABE Survey Form, in contrast, is a much more focused measure of reading skills.

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[10] Administering the mathematics and language sections of the TABE as well would have given a fuller picture of academic achievement, but time constraints made it infeasible to do so.

[11] The study of GAIN basic education found that the program was successful in increasing GED receipt for program registrants in all five counties that were in the impact study. In four of these counties, the impacts were statistically significant. In only one of these counties, however, did GAIN produce large and statistically significant impacts on literacy, as measured by the Test of Applied Literacy Skills (TALS).

A third, and related, hypothesis is that much of the instruction at the New Chance sites was geared toward preparing young women to pass the GED test. Once a young woman could read well enough to understand the questions asked on the test, her time in education classes was spent learning the specific subject matter tested, not in further improving her reading.

A final possible explanation is that New Chance increased GED attainment in large measure by increasing experimentals' opportunities to take the GED test. In other words, experimentals who read well enough to take the test and who had the requisite amount of subject knowledge to pass it were helped and pushed by New Chance staff to take the test, while controls who were equally academically able did not receive comparable assistance or encouragement.

Whatever the explanation (several may hold true simultaneously, and different explanations may apply to different sites), New Chance had no measured effect on the literacy of program enrollees. This finding is especially disturbing in that employers value not just education credentials but also the underlying skills these credentials are intended to represent.

IV. Impacts on the Receipt of Skills Training Credentials

A. Aggregate Impacts

Possession of a high school diploma or GED is often a prerequisite for entry into skills training; as has been noted, about 90 percent of both experimentals and controls who enrolled in a skills training program had one of these credentials in hand when they did so. Women in the experimental group, who were more likely than controls to earn an education credential, were also significantly more likely to have participated in occupational skills training over the 42-month period: 47.5 percent of experimentals versus 38.1 percent of controls (see Table 4.5).

Table 5.3 indicates, however, that virtually identical percentages of young women in both groups (25.2 percent of experimentals and 24.7 percent of controls) had earned a trade certificate or license (for example, as a nurse' s aide) by the 42-month interview. Why the experimentals' greater participation did not translate into a larger number of credentials is not clear.

About one in six sample members (18.1 percent of experimentals and 16.0 percent of controls, a difference that is not statistically significant) had earned both a secondary school credential and a trade license by the 42-month point and thus might have been deemed especially attractive to prospective employers.

B. Subgroup and Site Impacts

The story on subgroup and site impacts on attainment of training certificates is quickly told. New Chance produced a significant impact on attainment of a trade license among Hispanics and among those young women who had no previous work experience at baseline, but not for any other

Table 5.3

Impacts of New Chance on Receipt of Training Credentials Within 42 Months After Random Assignment

Outcome	Experimentals (%)	Controls (%)	Difference	p[a]
Received trade license by end of				
Month 6	5.4	7.2	-1.7	0.102
Month 18	13.6	14.2	-0.6	0.721
Month 30	20.1	19.2	0.9	0.623
Month 42	25.2	24.7	0.5	0.792
Received GED or high school diploma and trade license by end of				
Month 18	9.0	6.8	2.2 *	0.071
Month 42	18.1	16.0	2.1	0.203
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

The percentages shown are for all sample members, including the 6 percent who had already achieved a high school diploma or GED when they applied to the program.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

subgroup (see Table 5.4). There were no significant differences at any site in the proportions of experimentals and controls who held a trade license at the 42-month point.

C. Training Certificate Attainment and Program Participation

The instrumental-variables approach discussed earlier in the chapter was also used to estimate the statistical relationship between the amount of vocational skills training sample members received and their attainment of a trade license or certificate. The pattern of results was similar to the findings on attainment of education credentials; after correcting for selection bias, receiving a relatively small amount of skills training did not lead to a significantly different rate of credential attainment from receiving none at all, but receiving a good deal of training boosted the proportion receiving a training credential by a sizable and statistically significant margin (from 6 percent of those who received no skills training to 46 percent for those who received more than 20 weeks within the 42-month follow-up period).

V. New Chance and Other Programs Compared

Program impacts on literacy were not measured in LEAP, JOBSTART, and Project Redirection. Data on educational attainment and achievement were not reported for the dropout subgroup in the Teenage Parent Demonstration; for the full sample, the demonstration had no significant impact on GED receipt at the follow-up point.[12]

As has been noted earlier in the report, even when information is available it is often difficult to compare the results of other programs with those of New Chance because of differences in the program models, the populations served, the length of follow-up, and the program context. In Project Redirection, for example, 30 percent of both experimental and comparison group members who were out of school at baseline had obtained a high school diploma or GED by the five-year point (Polit and White, 1988). The population served by Project Redirection, however, was two and a half years younger on average than that enrolled by New Chance. LEAP enrollees were also younger than those in New Chance. At three years after random assignment, the LEAP program's effect on teens who were not enrolled in school upon entering the study was negligible: 18.6 percent of the experimentals and 22.1 percent of the controls reported on a survey that they had received a high school diploma or a GED (Long et al., 1996).

JOBSTART, which was directed toward male and female high school dropouts in approximately the New Chance age range, provides the closest comparison to New Chance. In JOBSTART, 42.0 percent of the young mothers in the experimental group and 26.7 percent in the control group received a GED or high school diploma within a 48-month follow-up period (Cave et al., 1993). In New Chance, it will be recalled, the figures were 51.9 percent for experimentals and 43.8 percent for controls within 42 months. Thus, one might conclude that while the absolute percentage of experimentals who earned a GED was somewhat greater in New Chance, the *impact*

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[12] Personal communication from Ellen Kisker, Mathematica Policy Research, March 7, 1994.

Table 5.4

Impacts of New Chance on Attainment of a Trade License Within 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Percentage with a Trade License		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.376
16-17	402	26.2	22.9	3.3	0.460		
18-19	997	24.1	26.4	-2.3	0.408		
20-22	678	26.3	23.3	3.0	0.376		
Ethnicity						--- *	0.066
Black, non-Hispanic	1,087	25.9	28.0	-2.1	0.422		
Hispanic	474	27.5	18.5	8.9 **	0.030		
White or other	515	21.7	23.2	-1.5	0.705		
Highest grade completed						2.0	0.643
10th or below	1,391	23.9	22.6	1.3	0.585		
11th or above	684	27.9	28.6	-0.6	0.852		
Interval since last attended regular high school						0.8	0.853
More than 2 years	1,093	25.0	23.7	1.3	0.634		
2 years or less	927	25.8	25.3	0.5	0.854		
TABE reading test score (grade equivalent)[c]						---	0.652
Below 6th grade	433	20.0	22.1	-2.0	0.636		
6th or 7th grade	492	24.9	20.2	4.7	0.253		
8th or 9th grade	566	27.2	26.5	0.7	0.848		
10th grade or above	583	27.5	28.8	-1.3	0.724		
Family received AFDC when sample member was growing up						---	0.291
Always	341	26.5	19.0	7.5	0.131		
Sometimes	970	22.3	23.7	-1.4	0.609		
Never	749	28.7	28.5	0.3	0.936		
Ever employed						11.2 **	0.020
Yes	1,646	25.3	27.1	-1.8	0.419		
No	433	24.7	15.3	9.4 **	0.027		
CES-D (depression) Scale[d]						---	0.345
0-15 (not at risk)	967	25.3	21.8	3.5	0.225		
16-23 (at some risk)	525	25.7	26.8	-1.1	0.775		
24-60 (at high risk)	582	24.6	27.5	-2.9	0.424		
Multiple risk score[e]						---	0.611
Low	871	25.9	26.8	-0.9	0.766		
Moderate	618	24.9	25.1	-0.2	0.967		
High	525	25.1	21.3	3.9	0.325		

(continued)

Table 5.4 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

(that is, experimental/control difference) was larger in JOBSTART (15.3 percentage points versus 8.1 percentage points in New Chance).[13]

In summary, New Chance had a positive and statistically significant effect on GED attainment, although not on the securing of training credentials. Furthermore, there is evidence that receiving more education and training services translated into higher rates of credential attainment. These findings suggest that programs need to focus on improving retention rates, so that enrollees participate long enough to realize a payoff.

New Chance emphasized education and training credentials not as ends in themselves but as the means to obtaining better-paying jobs. The extent to which this larger program goal was achieved is discussed in Chapter 7. First, however, it is important to examine other factors that may have influenced job-holding: living arrangements, fertility, and health and emotional well-being.

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[13] There were, however, subtle differences in the underlying populations served by the two programs that undermine the comparability of these results. The New Chance sample included a small percentage (6.1 percent) of young women who were high school graduates or GED holders upon entry into the research; they were included in the 51.9 percent statistic. JOBSTART enrolled dropouts exclusively and was targeted toward youth reading below the 8th-grade level, although it is estimated that about 20 percent of program enrollees were admitted under an eligibility "window" that permitted better-skilled readers to enroll. (Baseline reading scores were not measured for all JOBSTART enrollees.)

Furthermore, one cannot conclude that the same people who volunteered for one program might have volunteered for the other. New Chance applicants might have been especially attracted to a program geared exclusively toward young mothers. Had a program like JOBSTART been available to them, they might or might not have applied and might or might not have achieved impacts comparable to those registered in JOBSTART.

Finally, JOBSTART and New Chance were mounted in different communities (for the most part) and at different times. Contextual factors, including those that changed over time, must also be taken into account in trying to compare the effectiveness of different interventions.

Chapter 6

Living Arrangements, Fertility, Health, and Emotional Well-Being

I. Introduction

The self-sufficiency of unmarried young mothers can be influenced directly by activities that help develop their human capital, such as education and skills training, but many other factors also affect personal development and progress toward economic independence. This chapter focuses on aspects of the young mothers' lives that are not directly related to education and employment outcomes but that nevertheless play an important role in their overall well-being and ability to become self-sufficient.

A. Barriers to Economic Self-Sufficiency

Disadvantaged young women who become teenage mothers face a number of obstacles to attaining economic self-sufficiency. Even those young mothers who strive to leave the welfare rolls by working toward a GED certificate and gaining employment skills typically encounter a number of difficulties, including problems that result from factors beyond their control, such as having low reading skills or living in dysfunctional or nonsupportive families. Some problems are strongly associated with being young and poor and powerless—problems such as low self-esteem, depression, and fear of failing in a world where many have experienced failures before. Other difficulties represent the consequences of a young woman's own decisions, such as where and with whom to live or whether to have another baby.

In recognition of the difficult life circumstances of most of these young mothers, the New Chance program was designed to address the multiple barriers facing them. In addition to the components that directly offered skill-building activities, the New Chance model explicitly incorporated services and features intended to strengthen the young women's ability to overcome or avoid such barriers as unplanned pregnancies, health problems, housing problems, and personal or family problems.

This chapter examines impacts on several of the factors that were anticipated as potential threats to progress toward self-sufficiency and improved personal outcomes. Specifically, the chapter examines the effects of New Chance on living arrangements, fertility, family planning, health, and emotional well-being.

B. A Preview of the Findings

The results presented in this chapter suggest that New Chance did not have positive effects on the factors examined and in some cases had unanticipated negative effects. The experimental and control groups had nearly identical rates of pregnancies and births at 42 months after baseline, but the program accelerated the onset of pregnancies in the first half of the 42-month period. For example, at 20 months after random assignment, 58.5 percent of the experimentals and 54.6 percent of the controls had had at least one post-baseline pregnancy. The experimentals also had a higher rate of abortion than controls during the first two years after random assignment, which accounts for similar rates of post-baseline births for the two groups throughout the 42 months.

The program also had unintended effects on the living arrangements of the young mothers, effects that may have contributed to the early group differences in post-baseline pregnancy. At 18 months after random assignment, a higher percentage of women in the experimental group (22.9 percent) than in the control group (18.2 percent) were living with a husband or partner, and a lower percentage of experimentals (27.9 percent) than controls (35.5 percent) were living with a parent or grandparent. By the 42-month point, the percentages of women in the two groups who were living with parents or partners were comparable; apparently controls did not move away from their parents as quickly as did experimentals, but eventually they caught up. On the other hand, some program effects on living arrangements persisted throughout the follow-up period and suggest greater instability among experimentals; at the 42-month interview, women in the experimental group were more likely to be living in one of an assortment of "other" arrangements (such as living with nonrelatives), were more likely to be living without one of their children, and had moved more often than their counterparts in the control group.

The two groups were similar at both points of follow-up with respect to self-rated health, days of illness, and health-related behaviors such as smoking, drinking, and using drugs. A higher percentage of women in the experimental group (24.4 percent) than in the control group (20.3 percent), however, had been hospitalized since random assignment for a reason not related to childbirth.

Reports of stress and depression were high in both groups at both follow-up interviews. Women in the experimental group, however, scored higher than controls on a widely used depression scale at the 42-month point. Depression scores for both experimentals and controls improved during the follow-up period, but scores improved more for controls than for experimentals. Also, a higher percentage of experimentals (39.4 percent) than controls (33.2 percent) reported feeling highly stressed in the month prior to the 42-month interview. At 42 months, women in the experimental group (69.7 percent) were less likely than those in the control group (73.7 percent) to say they were satisfied with their standard of living. These unexpected negative impacts on indicators of emotional well-being were especially substantial among sample members who were at high risk of depression when they applied to the program.

On the whole, the findings in this chapter raise concerns about the young women's personal development and about New Chance's impacts in this area, particularly for those who had emotional problems when they first applied to the program. Although the dynamics of what happened are difficult to understand, it appears that for at least some of the young mothers, New Chance increased instability in living arrangements that may in turn have affected a wide range of other outcomes. The program may also have raised expectations about improved life outcomes that, when left unfulfilled, had negative effects on the women's emotional well-being.

II. Living Arrangements, Marriage, and Cohabitation

The living arrangements of poor young mothers have been the focus of considerable policy debate, stemming in part from beliefs about how living arrangements influence the young women's options and behaviors. Those who argue that young welfare mothers should be required to live with a parent perceive such a requirement to be a deterrent to the establishment of an independent welfare-supported household headed by a teenage mother; many proponents also believe that a multigenerational household offers guidance and support as well as the possibility for convenient and inexpensive child care that can facilitate the young mother's pursuit of educational and job-related activities. On the other hand, opponents of such requirements point out that many young women who move out of their parents' households do so to escape dysfunctional and often violent family situations.

The New Chance program came at a point in the young women's lives when living arrangements are typically in a state of fiux—that is, in young adulthood, when many people are testing alternatives to living at home (DaVanzo and Goldsheider, 1990; Thornton, Young-deMarco, and Goldsheider, 1993). New Chance may have accelerated this process. This section explores whether it did.

A. Aggregate Program Impacts on Living Arrangements

As Chapter 3 pointed out, changes in living arrangements were common among New Chance sample members. Many sample members moved out of their parents' households during the 42-month follow-up period, and the percentage of women living with a male partner nearly tripled over the 42 months following baseline (from 11 percent to 31 percent). By the end of the follow-up period only about 20 percent of the women were still living with a parent or grandparent. The most common living arrangement was living alone with children with no one else in the household (36 percent).

Although New Chance did not specifically intend to have effects on living arrangements or marriage, it is conceivable that an intensive and comprehensive program like New Chance may have had effects in this area. For example, some program staff provided assistance with housing arrangements, possibly giving some participants an opportunity to leave their parents' households.[1] Also, some young mothers who were offered free child care through the program may have felt better able to establish an independent household because of a diminished need for parental assistance with child care.

Table 6.1 shows that there were several statistically significant differences between experimental and control group members with respect to living arrangements after random assignment, and that these differences varied over time. At the 18-month follow-up interview,

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[1] The programs intervened to help participants with housing problems primarily when staff perceived that the young women were in crisis (for example, when a parent evicted them or when there was concern about physical or sexual abuse in the household).

Table 6.1

Impacts of New Chance on Living Arrangements at or Within 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Living with a husband or partner (%)				
at baseline	11.0	11.1	-0.1	
at 18-month follow-up	22.9	18.2	4.2 **	0.016
at 42-month follow-up	30.7	31.7	-1.0	0.625
Living with a parent or grandparent (%)				
at baseline[b]	36.3	34.0	2.3	
at 18-month follow-up	27.9	35.5	-7.6 ***	0.000
at 42-month follow-up	21.3	20.4	1.0	0.606
Living with children only (%)				
at baseline	32.2	31.0	1.2	
at 18-month follow-up	36.3	34.7	1.6	0.466
at 42-month follow-up	35.7	38.9	-3.2	0.139
Living in another arrangement (e.g., with friends, alone, in an institution) (%)				
at 18-month follow-up	12.8	11.6	1.2	0.441
at 42-month follow-up	12.3	9.0	3.3 **	0.027
Living without any of her children (%)				
at baseline	0.7	1.2	-0.5	
at 18-month follow-up	2.4	2.0	0.4	0.578
at 42-month follow-up	4.9	2.8	2.1 **	0.026
Average number of household members				
at baseline	4.1	4.2	-0.1	
at 18-month follow-up	4.1	4.3	-0.2 **	0.040
at 42-month follow-up	4.3	4.1	0.2 **	0.043
Average number of times moved between birth of focal child and 42-month follow-up[c]	4.0	3.7	0.3 **	0.042
Has had trouble in past 12 months finding a good place to live, at 42-month follow-up (%)	41.9	37.5	4.4 *	0.061
Sample size	1,401	678		

(continued)

Table 6.1 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The baseline survey did not include living with a grandparent in this question.

[c] For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

significantly more women in the experimental group (22.9 percent) than in the control group (18.2 percent) reported living with a partner or husband. This difference disappeared by 42 months after random assignment, when about one third of the members of each group were living with a partner. Conversely, significantly fewer experimentals (27.9 percent) than controls (35.5 percent) were living with a parent or a grandparent at 18 months. At the 42-month interview, a similar percentage of both groups (just over 20 percent) were still living with a parent figure.[2]

Comparable percentages of women in the two groups were living alone with their children at both follow-up interviews—about one third of the mothers in each group. At the final interview, a minority of women—but more experimentals (12.3 percent) than controls (9.0 percent)—were living in a variety of other arrangements, such as living with friends or with other relatives. The difference was statistically significant. It should also be noted that even within the "other" category there were group differences; experimentals were more likely than controls to be living in other arrangements that did not include family members (not shown in the table).[3]

The results shown in Table 6.1 also indicate that a significantly higher percentage of experimentals (4.9 percent) than controls (2.8 percent) were living without any of their children at the time of the 42-month interview. Also, significantly more experimentals (12.5 percent) than controls (10.0 percent) were living without at least one of their children (not shown in the table). Information is available from the survey on the whereabouts of 265 experimental group children and 105 control group children who were not living with their mothers at the 42-month interview. Of these children, a higher percentage in the experimental group (10.9 percent) than in the control group (4.8 percent) were in foster care. The most common arrangement for controls was having a child who lived with his or her father (31.4 percent for controls versus 26.4 percent for experimentals). Among the experimentals, the children were most likely to be living with a maternal grandparent; this arrangement was also common for those in the control group (27.2 percent of the experimentals, 29.5 percent of the controls).

As is shown in Table 6.1, at 42 months after random assignment experimentals were living in significantly larger households than controls—despite the absence of more of their children, and despite the fact that experimentals had lived in significantly *smaller* households at 18 months. The 42-month difference is accounted for by a larger number of adults, not children, in experimentals' households (not shown).

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[2] At 42 months, similar percentages of experimentals and controls (about 49 percent) reported the same *type* of living arrangement as they had reported at the 18-month interview (not shown in the table). Although controls were less likely than experimentals to be living with a partner at the 18-month point, those who were in such an arrangement were more likely to still be in that arrangement at the 42-month interview (66 percent) than their counterparts in the experimental group (56 percent), although it is unknown whether the partner was the same.

[3] For example, experimentals were more likely than controls to be living with friends, with no relative present (28.6 percent versus 23.3 percent, respectively, of those in the "other" category); to be living completely alone without any children (15.5 percent versus 10.0 percent); or to be in jail (5.4 percent versus 1.7 percent). By contrast, of those in "another" arrangement, experimentals were less likely than controls to be with relatives, such as siblings, nieces, and/or nephews (33.3 percent versus 38.3 percent), or aunts, uncles, and/or cousins (21.4 percent versus 25.0 percent).

Mothers in the experimental group reported that they had moved more often since the birth of the focal child than those in the control group. ("Move" refers to physical moves, not to changes in household composition.) Finally, at the final interview more experimentals (41.9 percent) than controls (37.5 percent) reported that they had had difficulty finding a good place to live in the previous 12 months. Both of these experimental/control differences were statistically significant.

Perhaps not surprisingly, the various outcomes for which there were program impacts were interconnected and suggest greater residential stability for controls than for experimentals. For example, the number of moves was correlated with housing problems; 34.5 percent of those with two or fewer moves, compared with 49.8 percent of those with five or more moves, said they had had trouble finding a good place to live in the past year. The number of moves also varied by living arrangement at the 42-month interview. The lowest average number of moves (3.2) was found for women who were living with a parent or grandparent; the highest average (5.1) was among women who were living in an "other" arrangement. Among the women in the control group, those living in an "other" arrangement were about as likely as those living with parents, with partners, and with children alone to say that they had had trouble in the previous year finding a good place to live (36 percent versus 38 percent, respectively). Women in the experimental group who lived in an "other" arrangement, however, were substantially more likely to say they had had trouble finding a good place to live (60 percent) than those in a more typical arrangement (40 percent).

Despite the impacts on living arrangements, the program had no significant effects on marital status or on reported relationships with men. As is shown in Table 6.2, a similar percentage of women in the experimental group (13.0 percent) and in the control group (12.3 percent) were married at the final interview, in both cases reflecting a substantial increase from the 3 percent who were married at baseline. About three-fourths of the women in both groups had never been married. A comparable percentage in the two groups (about 70 percent) reported being in a steady relationship with (or living with) a man at the time of the 42-month follow-up.

Table 6.2 also shows several nonexperimental comparisons for questions asked of non-married women at the final interview. Fewer than half these women (about 44 percent in both groups) wanted to be married within the next two years. Nearly one out of five said they preferred to remain unmarried. Among those women who were either married or in a steady relationship, the average rating of the quality of their relationship, on a scale from 0 ("extremely unhappy") to 10 ("perfectly happy"), was fairly positive—an average of nearly 8 in both groups.

In summary, New Chance had a number of short-term and longer-term impacts on the living arrangements of the young mothers. The findings suggest that New Chance may have

Table 6.2

Impacts of New Chance on Marriage and Relationships at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Married (%)				
at baseline	2.8	3.1	-0.3	
at 18-month follow-up	7.8	7.2	0.6	0.623
at 42-month follow-up	13.0	12.3	0.6	0.688
Single, never married (%)				
at baseline	90.7	89.2	1.5	
at 18-month follow-up	85.0	84.5	0.6	0.645
at 42-month follow-up	75.9	77.8	-1.9	0.231
Divorced/widowed/separated (%)				
at baseline	6.5	7.7	-1.2	
at 18-month follow-up	7.1	8.3	-1.2	0.230
at 42-month follow-up	11.2	9.8	1.3	0.272
In a steady relationship with one man at 42-month follow-up (%)	69.7	69.8	-0.1	0.962
Among those who never married:				
Would like to get married within next 2 years (%)	43.7	43.8	-0.1	0.960
Would like to get married, but not in next 2 years (%)	37.4	38.3	-0.9	0.739
Would prefer not to get married at all (%)	18.9	17.9	1.0	0.631
Average rating of quality of relationship with current husband or partner (0-10)	7.9	7.8	0.2	0.191
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

Distributions may not add to 100.0 percent because of rounding.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

accelerated the move away from the parental home and that this move in turn may have reduced the stability of the sample members' living arrangements.[4]

B. Subgroup and Site Impacts on Living Arrangements

At the 18-month follow-up interview, program impacts on living arrangements were fairly pervasive across subgroups and sites. At the 42-month interview, however, the situation was somewhat different. For example, as is shown in Table 6.3, the program impact at 42 months on reported difficulty in finding a good place to live was observed primarily among subgroups of women with greater degrees of disadvantage at baseline: those at some or high risk of depression and those with very low reading skills. A similar pattern was observed with respect to subgroup impacts on the number of moves since the birth of the focal child (not shown).[5] Thus, it appears that the most vulnerable groups of women—and perhaps ones whose family situations were least stable to begin with—were most likely to have their living arrangements affected by New Chance.

Site impacts on living arrangements at the 42-month interview tended to be small and inconsistent.[6] They do not suggest any clues regarding the underlying cause of the aggregate program impacts.

III. Fertility and Contraception

When the young mothers applied to the New Chance program, the majority of them had only one child, said they expected no more children, and reported that they were using contraceptives to postpone or prevent further childbearing. Yet many studies have found that a high percentage of teenage mothers have an early repeat pregnancy. For example, reports from large-scale surveys have generally found that about 40 percent of teenage mothers become pregnant again within 24 months of delivering their first child (Koenig and Zelnik, 1982; Mott, 1986).

In recognition of these high rates of early repeat pregnancy, programs for teenage mothers almost always offer family planning services, and most have the postponement of subsequent pregnancies as an explicit goal. Yet there is no evidence that any large-scale teen parent program

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[4] An instrumental variables analysis (described in detail in Chapter 8) suggested that the increased availability of child care for experimentals may have played a role in the young mothers' moves. According to this analysis, which controlled for maternal characteristics associated with child care use, women whose child was ever in a day care center in the first 18 months after random assignment moved more often than women whose child was never in a day care center. Each month the child spent in a child care center was associated with an additional 0.12 moves. As is discussed in Chapter 8, New Chance substantially increased the availability and use of day care.

[5] Surprisingly, the impact on " living without one of her children" was more likely to occur among women who were relatively advantaged at random assignment. Experimental group women who were not depressed, who already had a diploma or GED at baseline, and who had a 10th grade reading level or better were more likely than their control group counterparts to have a child living elsewhere (not shown).

[6] The program impact on difficulty in finding a good place to live was statistically significant in three sites: Allentown, Harlem, and Philadelphia. Despite these site-specific effects, however, the between-site impact difference was not significant (not shown).

Table 6.3

Impacts of New Chance on Sample Members' Difficulty Finding Good Housing a Year Prior to the 42-Month Follow-Up, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Had Difficulty Finding Good Housing: Experimentals (%)	Had Difficulty Finding Good Housing: Controls (%)	Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
Age (years)						---	0.312
16-17	402	39.8	36.2	3.6	0.507		
18-19	997	40.6	39.1	1.5	0.651		
20-22	678	45.4	35.7	9.6 **	0.021		
Ethnicity						---	0.981
Black, non-Hispanic	1,087	42.5	38.5	4.0	0.204		
Hispanic	474	40.8	36.1	4.8	0.342		
White or other	515	41.6	36.5	5.1	0.302		
Highest grade completed						-1.8	0.714
10th or below	1,391	41.3	37.6	3.7	0.196		
11th or above	684	43.2	37.7	5.6	0.175		
Interval since last attended regular high school						-1.5	0.760
More than 2 years	1,093	42.0	38.5	3.5	0.283		
2 years or less	927	41.4	36.4	5.0	0.158		
TABE reading test score (grade equivalent)[c]						---	0.474
Below 6th grade	433	42.3	31.6	10.7 **	0.038		
6th or 7th grade	492	39.9	37.2	2.7	0.586		
8th or 9th grade	566	40.1	39.9	0.3	0.951		
10th grade or above	583	45.5	40.1	5.4	0.225		
Family received AFDC when sample member was growing up						---	0.966
Always	341	46.7	43.6	3.1	0.604		
Sometimes	970	42.3	37.5	4.9	0.149		
Never	749	39.4	35.1	4.3	0.280		
Ever employed						5.9	0.306
Yes	433	41.7	32.8	8.9 *	0.081		
No	1,646	42.0	39.0	3.0	0.252		
CES-D (depression) Scale[d]						--- *	0.090
0-15 (not at risk)	967	37.6	38.6	-1.0	0.774		
16-23 (at some risk)	525	42.8	31.9	10.9 **	0.019		
24-60 (at high risk)	582	48.8	41.3	7.5 *	0.087		
Multiple risk score[e]						---	0.304
Low	871	37.7	37.6	0.2	0.967		
Moderate	618	43.9	36.0	7.9 *	0.062		
High	525	46.9	39.8	7.2	0.131		

(continued)

Table 6.3 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as ***= 1 percent; **= 5 percent; *= 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

has been successful in achieving this goal. For example, in Project Redirection, nearly half the members of both the experimental and the comparison groups had a subsequent pregnancy within 24 months after baseline; at the five-year follow-up of the Project Redirection sample, women in the experimental group had given birth to a significantly higher average number of children (Polit, Quint, and Riccio, 1988).

High rates of early repeat pregnancy and birth are of concern to program operators, because there is considerable evidence that having a second child further reduces teenage mothers' participation in school, training, and employment (see, for example, Furstenberg, Brooks-Gunn, and Morgan, 1987; Horwitz et al., 1991; Polit, Quint, and Riccio, 1988). Thus, New Chance program staff were explicitly expected to discourage early subsequent pregnancies among program participants. Several components of the New Chance model were designed to help young mothers make sound decisions regarding contraception and childbearing. Family planning classes were offered at all sites, and some sites offered contraceptives directly. Life skills classes emphasized the importance of decision-making skills and also sought to empower young women in various ways, including enhancing their control over reproductive events. Because repeat pregnancy had been a concern in the Project Redirection demonstration, family planning issues were addressed in technical assistance conferences with New Chance staff prior to implementation. While programs did not discourage further childbearing per se—nor did they directly encourage abortion as a means of postponing childbearing—program staff (at least in theory) endorsed postponement of further pregnancies until the young mothers had made some progress toward self-sufficiency.[7]

This section describes fertility and contraception in the New Chance sample during the 42 months of follow-up and examines program impacts in these areas. Given the program model, it was hoped that young women in the experimental group would be more likely than controls to use effective contraceptive methods and would be less likely to have experienced an early post-baseline pregnancy. Program effects on subsequent pregnancy and GED attainment considered simultaneously are also examined to see whether the positive impacts on GED attainment were observed primarily for those who avoided a subsequent pregnancy.

A. Aggregate Program Impacts on Fertility and Contraception

Most young mothers in the New Chance sample (75 percent) had another pregnancy during the follow-up period and just over half (55 percent) had another baby. Figure 6.1 shows that the New Chance program did not reduce the rate of pregnancies or childbearing in the post-baseline period. The cumulative percentage of young mothers who became pregnant after random assignment rose steadily for both groups. In many of the 42 months of follow-up, women in the experimental group had a somewhat higher rate of post-baseline pregnancy than their counterparts in the control group, with differences reaching statistical significance for 9 of the 42 months of

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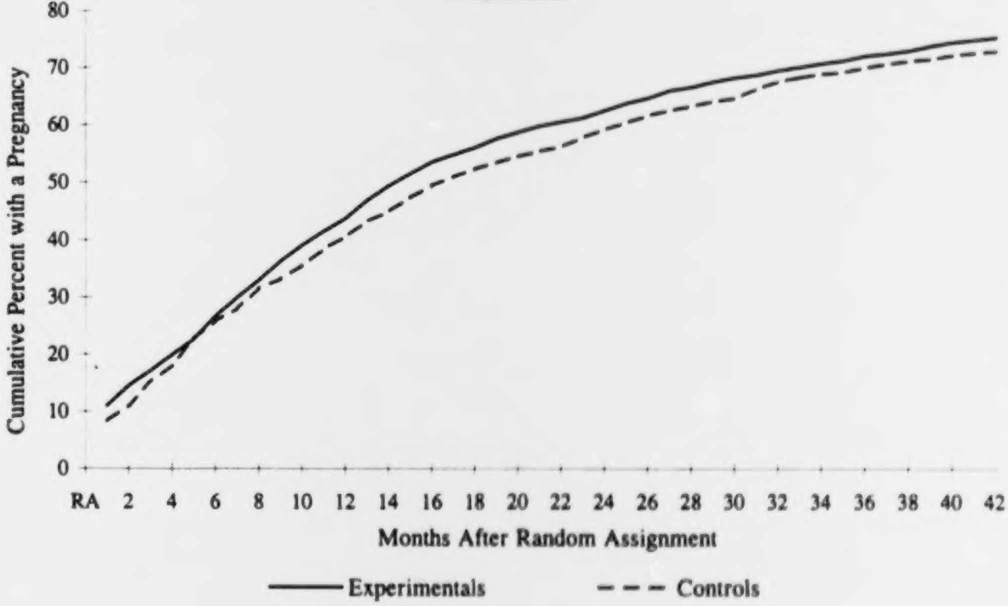
[7] A question in the 18-month survey explicitly asked respondents to rate the extent to which New Chance staff emphasized postponing another pregnancy. On a scale from 0 ("not at all") to 10 ("the most possible"), the mean rating was 7.2. This average suggests that many young mothers were aware of a pregnancy-postponement message but that the message was not perceived to be as powerful as it might have been.

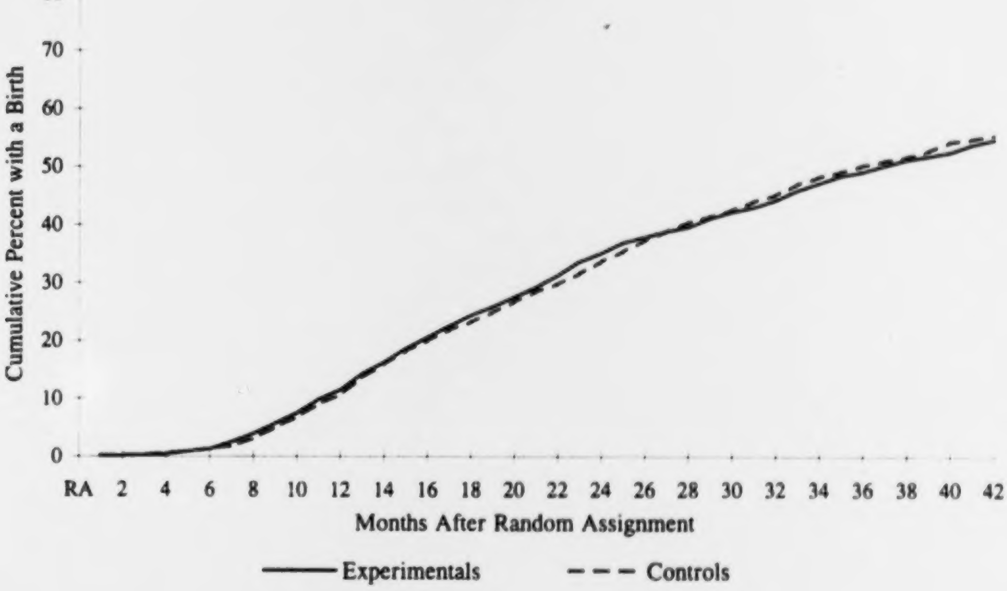
Figure 6.1

Cumulative Rates of Pregnancy and Birth for New Chance Sample Members Within 42 Months After Random Assignment

Pregnancies



Births



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.6 for data corresponding to figures.

follow-up (months 1–2, 14–16, and 19–22). By the 42-month point, however, the cumulative pregnancy rate for the two groups was not significantly different.

With respect to post-baseline births, the patterns over time for the two groups were almost identical, and differences were never statistically significant. The apparent discrepancy between impacts on pregnancies and impacts on births is accounted for by the fact that for 11 of the 42 months of follow-up, experimentals had a significantly higher rate of post-baseline abortion than controls, peaking in month 23, by which time 13.5 percent of the experimental group women and 9.9 percent of the control group women had had a post-baseline abortion (not shown). At 42 months after random assignment, however, the program impact on the cumulative abortion rate was not significant.

Table 6.4 presents findings for several fertility-related outcome measures. What is most conspicuous about this table is the absence of any statistically significant differences between the two groups. Overall, the childbearing histories of the research groups were quite similar, despite the somewhat higher rates of pregnancy among experimentals in the early months after random assignment. At the final interview, most New Chance sample members were still caring for toddlers; the average age of the youngest child of both experimentals and controls was under 3 years.

Table 6.4 also shows that the two groups were equally likely to have had a planned or wanted pregnancy during the 42-month follow-up period (26.2 percent of the experimentals and 25.5 percent of the controls)—and equally likely to have had an *un*planned, mistimed, or unwanted pregnancy (65.0 percent of the experimentals and 64.7 percent of the controls).[8] Of course, retrospective accounts of planning a pregnancy may well be subject to recall bias. Moreover, among young mothers, it is possible that "planning" or "wanting" a pregnancy is not always conscious. In any event, when asked at the final interview how upset they were about their most recent pregnancy, on a scale from 0 ("not at all upset") to 10 ("extremely upset"), the average rating was under 5. Women who said the pregnancy came at about the right time had an average rating of 1.0, while those who said they had never wanted to become pregnant again had an average rating of 7.5.

The average spacing between the last-terminated pregnancy at baseline and the onset of a subsequent pregnancy was estimated to be similar in the two groups: 36.1 months for experimentals and 38.2 months for controls.[9] The difference, however, which was statistically significant, suggests that New Chance accelerated sample members' first post-baseline pregnancy. At any time after baseline—including the final interview—between 10 percent and 25 percent of the women were

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[8] In the 18-month interview, women were asked if pregnancies initiated after random assignment were planned or unplanned. In the 42-month interview, the women were asked whether any pregnancies initiated after the first follow-up interview had begun sooner than they had wanted (that is, were mistimed) and whether the pregnancies were unwanted (that is, the women had not wanted to get pregnant again).

[9] These numbers were produced with a Tobit estimator to correct for truncation of the outcome variable at the end of the 42-month follow-up period (that is, the researchers' inability to observe the outcome beyond the follow-up period) for sample members who did not have an additional birth during the follow-up period.

Table 6.4

Impacts of New Chance on Pregnancy and Childbearing at or Within 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Had a post-baseline pregnancy (%)				
by month 18	55.9	52.3	3.6	0.119
by month 42	75.2	72.8	2.3	0.246
Average total number of pregnancies				
at baseline	2.0	1.9	0.1	
at 18-month follow-up	2.6	2.6	0.5	0.305
at 42-month follow-up	3.4	3.4	0.1	0.841
Had a post-baseline birth (%)				
by month 18	24.4	23.3	1.1	0.569
by month 42	54.7	55.3	-0.7	0.767
Average total number of live births				
at baseline	1.4	1.5	-0.1	
at 18-month follow-up	1.8	1.7	0.0	0.433
at 42-month follow-up	2.2	2.2	0.1	0.169
Average age of youngest child (years)				
at baseline	1.2	1.2	0.0	
at 18-month follow-up	1.6	1.7	-0.1	0.136
at 42-month follow-up	2.7	2.8	-0.1	0.483
Had a post-baseline abortion (%)				
by month 18	11.5	9.6	1.8	0.203
by month 42	17.4	14.8	2.5	0.129
Ever had an abortion (%)				
at baseline	23.9	22.5	1.4	
at 18-month follow-up	31.6	29.7	2.0	0.280
at 42-month follow-up	40.6	38.6	2.0	0.290
Had one or more planned/wanted pregnancies (%)				
in months 1-18	8.2	6.5	1.7	0.196
in months 19-42	21.3	21.1	0.2	0.911
in months 1-42	26.2	25.5	0.7	0.714
Had one or more unplanned/mistimed/ unwanted pregnancies (%)				
in months 1-18	49.1	48.0	1.1	0.636
in months 19-42	40.9	42.6	-1.6	0.470
in months 1-42	65.0	64.7	0.3	0.889

(continued)

Table 6.4 (continued)

Outcome	Experimentals	Controls	Difference	p[a]
Average months between last terminated pregnancy at random assignment and onset of next pregnancy[b]	36.1	38.2	-2.2 *	0.070
Percent pregnant				
at baseline	0.0	0.0	0.0	
at 18-month follow-up	15.0	14.5	0.5	0.784
at 42-month follow-up	9.6	11.9	-2.3	0.113
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] These estimates were generated using a Tobit estimator to correct for truncation at the end of the follow-up period.

pregnant. Thus, it appears that like many comparable interventions for young mothers, the program had little long-term effect on fertility, although there were some short-term effects in an unintended direction on post-baseline pregnancies.

Table 6.5 shows program impacts on birth control outcomes. Despite the fact that at all three interviews approximately half of the sample members reported not wanting any more children, a persistent 20–30 percent of the sample failed to use contraception regularly. At the 42-month interview, there were no significant program effects on birth control outcomes; members of the two groups were equally likely to be abstinent, using contraceptives irregularly, or using contraceptives regularly.[10] At the first follow-up interview, however, two statistically significant differences were observed. At 18 months after random assignment, significantly more controls (40.5 percent) than experimentals (36.6 percent) were sexually active and using contraceptives regularly, and more experimentals (30.7 percent) than controls (25.1 percent) were sexually active but not using contraceptives regularly. These short-term negative impacts on contraceptive use, which are consistent with the short-term impact on the pregnancy rate, disappeared by the final interview. Over time, both groups increased their use of a prescription or surgical method of contraception, with just over half the women in both groups using such a method at the end of the follow-up period. About 30 percent of both groups were using a method that provided long-term protection against a pregnancy, such as tubal ligation,[11] NORPLANT®, or Depo-Provera. However, 59.8 percent of the women in both groups said they had had a sexual contact in the previous two months that was unprotected against a sexually transmitted disease.[12]

Table 6.5 also shows that the two groups were similar in their fertility expectations: nearly two thirds of the women in both groups said at the final interview that they expected to have no more children. About 15 percent of the nonpregnant women who expected to have more children said they expected to have another child within the next 12 months; however, about one third of the nonpregnant women said they expected to have another child 5 or more years later (not shown).

When asked in the 42-month interview how upset they would be if they became pregnant the next month (on a 0 to 10 scale, where 0 means "not at all" and 10 mean "the most possible"), the average "upset" rating in both groups suggests a fair amount of concern about another pregnancy (7.4 mean for both groups). The mean on this rating masks a preponderance of ratings at

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[10] Among the women who were sexually active and not pregnant at the 42-month follow-up (69 percent of the sample), the most commonly used methods of contraception were as follows (the percentages total more than 100 percent because a number of young women used multiple methods): condoms (39.2 percent), birth control pills (19.7 percent), tubal ligation (15.5 percent), withdrawal (14.1 percent), Depo-Provera (10.4 percent), NORPLANT® (8.8 percent), rhythm (5.4 percent), and foams, jellies, or suppositories (4.7 percent).

[11] The majority of women who reported having had a tubal ligation (13 percent of the sample) had had three or more children.

[12] This finding is similar to findings from a 1990 survey of a nationally representative sample of sexually experienced unmarried women; only 30 percent of the women reported using condoms for protection against sexually transmitted diseases every time they had intercourse (Anderson, Brackbill, and Mosher, 1996).

Table 6.5

Impacts of New Chance on Birth Control and Fertility Expectations at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Sexually abstinent, not pregnant (%)				
at baseline	33.0	33.9	-0.9	
at 18-month follow-up	17.7	19.8	-2.1	0.258
at 42-month follow-up	20.0	17.8	2.1	0.259
Sexually active, using contraceptives regularly (%)[b]				
at baseline	45.1	45.1	0.0	
at 18-month follow-up	36.6	40.5	-3.9 *	0.094
at 42-month follow-up	41.4	44.0	-2.6	0.254
Sexually active, not using contraceptives regularly (%)[b]				
at baseline	21.0	20.0	1.0	
at 18-month follow-up	30.7	25.1	5.6 **	0.012
at 42-month follow-up	29.0	26.2	2.8	0.187
Used a prescription/surgical method of birth control during prior two months (%)[c]				
at 18-month follow-up	33.0	31.4	1.6	0.488
at 42-month follow-up	51.2	53.1	-1.9	0.447
Had a tubal ligation, NORPLANT® implant, or Depo-Provera shot at 42-month follow-up (%)	30.2	31.0	-0.7	0.727
Ever had sex unprotected against a sexually transmitted disease during past two months at 42-month follow-up (%)	59.8	59.8	0.0	0.994
Expects to have no more children (%)				
at baseline	64.0	64.3	-0.3	
at 18-month follow-up[d]	53.6	55.6	-1.9	0.563
at 42-month follow-up	61.2	64.2	-3.1	0.170
Rating of how upset woman would be if a pregnancy occurred next month[e]				
at 18-month follow-up	7.7	7.3	0.4	0.123
at 42-month follow-up	7.4	7.4	0.0	0.891
Sample size	1,386	667		

(continued)

Table 6.5 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for 2,053 sample members for whom there were 42-month follow-up survey data on birth control and fertility questions, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] A respondent who reported using contraception at each intercourse and/or said that she always took a birth control pill when she was supposed to was considered to be using contraception regularly.

[c] Includes tubal ligation, birth control pills, NORPLANT®, Depo Provera, and the IUD.

[d] The question on future childbearing expectations was asked of half the 18-month follow-up sample, selected at random.

[e] Respondents were asked to rate how upset they would be on a scale of 0 ("not at all upset") to 10 ("the most possible").

the extremes, however; 60 percent of the women gave a rating of 10, and 16 percent gave a rating of 0.[13]

B. Subgroup and Site Impacts on Fertility and Contraception

New Chance appears to have had a modest but significant effect on increasing the rate of pregnancies in the early post-baseline period for the aggregate sample. While this impact was not statistically significant for all subgroups examined, it was fairly pervasive; in virtually every subgroup, experimentals had a higher rate of post-baseline pregnancy at the 18-month point than controls (not shown).

Similarly, the *absence* of 42-month impacts for the fertility and contraceptive outcomes for the aggregate sample was echoed in the subgroup results. There were only a handful of significant subgroup experimental/control differences with respect to having had a pregnancy, birth, or abortion during the 42 months of follow-up, and the subgroup differences were small and seemingly random. For example, as is shown in Table 6.6, there were no significant impacts on having had a birth within 42 months after random assignment for *any* of the subgroups. There were also few subgroup impacts on use of contraceptives, and these also failed to suggest a consistent pattern (not shown).

Program impacts on fertility at the site level were somewhat erratic over time. For example, at the 18-month point San Jose experimentals had fewer post-baseline pregnancies and significantly fewer unplanned pregnancies than controls at that site, but this difference was not observed at the 42-month point. The converse was true in Portland, where 18-month results that favored the control group disappeared by the end of the study. The only site result that persisted over time was in Detroit; at both 18 months and 42 months, significantly more experimentals than controls in that site had had a post-baseline pregnancy. Moreover, the experimentals in Detroit had had a significantly larger average number of births than the controls by the final interview. (The across-site variation in the impacts on this outcome was almost statistically significant, p = .13.)

D. Fertility and Program Participation

As is explained in Chapter 5, it is possible to compare outcomes for women with different patterns and levels of service receipt and to use the experimental assignment variable to adjust these comparisons for possible selection bias.[14] When such an analysis is done, no relationship appears between overall participation in education and training and the probability that sample members would give birth during the 42-month follow-up period. It does appear, however, that family

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[13] Women who said at the final interview that they never used contraception had an average "upset" rating of 5.3, while women who reported using contraception all the time had an average rating of 7.7, suggesting that the ratings were reasonably consistent with behavior. It might be noted, however, that the correlation between the "upset" rating at the 18-month interview and a pregnancy initiated subsequently was weak ($r = -.05$, p = .12).

[14] This adjustment is based on the use of an instrumental variables estimator (see Chapters 2 and 5). The experimental assignment variable is used to predict receipt of services. The predicted values (which are free of selection bias) are then used to estimate the effect of service receipt on subsequent outcomes.

Table 6.6

Impacts of New Chance on Childbearing Within 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Percentage who Gave Birth		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.603
16-17	402	57.3	62.6	-5.3	0.323		
18-19	997	55.4	54.4	1.0	0.755		
20-22	678	51.8	52.5	-0.7	0.861		
Ethnicity						---	0.826
Black, non-Hispanic	1,087	57.9	57.4	0.5	0.879		
Hispanic	474	48.8	51.9	-3.1	0.524		
White or other	515	53.3	54.0	-0.7	0.885		
Highest grade completed						-4.3	0.385
10th or below	1,391	54.7	56.9	-2.1	0.449		
11th or above	684	54.4	52.2	2.1	0.594		
Interval since last attended regular high school						2.8	0.556
More than 2 years	1,093	53.7	53.2	0.5	0.880		
2 years or less	927	55.9	58.2	-2.3	0.509		
TABE reading test score (grade equivalent)[c]						---	0.424
Below 6th grade	433	63.1	57.3	5.8	0.251		
6th or 7th grade	492	54.4	59.9	-5.5	0.251		
8th or 9th grade	566	52.2	54.1	-2.0	0.650		
10th grade or above	583	51.2	50.8	0.4	0.920		
Family received AFDC when sample member was growing up						---	0.953
Always	341	56.6	57.3	-0.6	0.916		
Sometimes	970	55.4	56.8	-1.4	0.667		
Never	749	52.7	52.5	0.2	0.969		
Ever employed						0.5	0.929
Yes	1,646	53.8	54.7	-1.0	0.702		
No	433	57.8	58.3	-0.5	0.924		
CES-D (depression) Scale[d]						---	0.160
0-15 (not at risk)	967	53.4	57.9	-4.5	0.191		
16-23 (at some risk)	525	52.5	53.6	-1.2	0.796		
24-60 (at high risk)	582	59.1	53.1	6.0	0.162		
Multiple risk score[e]						---	0.523
Low	871	55.0	54.8	0.2	0.966		
Moderate	618	53.8	57.4	-3.6	0.384		
High	525	56.0	52.6	3.4	0.459		

(continued)

Table 6.6 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. Although it is possible to assess the statistical significance of variation across multiple subgroups, these differences were not statistically significant.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

planning services may have reduced birth rates during the 42-month follow-up period, as sample members who attended more than 10 family planning sessions were somewhat less likely to have given birth, both at 18 and at 42 months (not shown).

E. Fertility in Relation to Other Outcomes

Given the concern that another pregnancy could adversely affect the young mothers' ability to complete the New Chance program and move forward with their lives, it is useful to consider subsequent pregnancies in this sample in relation to GED attainment. For example, did program impacts on GED attainment occur only in the absence of another pregnancy? Conversely, was the short-term negative impact on subsequent pregnancies associated with failure to achieve a GED? Another important question concerns the relation between post-baseline pregnancies and living arrangements at follow-up. This section examines group differences with respect to program impacts on pregnancies during the follow-up period in relation to educational attainment and living arrangements.

1. Fertility and education. Table 6.7 shows the percentage of experimental and control group women who had obtained their GEDs or diplomas by the time of the 18-month interview (top panel) or the 42-month interview (bottom panel) among women who had had a post-baseline pregnancy or birth and those who had not. This table shows that fertility outcomes and educational attainment were strongly related for both research groups at both points; women who had had another pregnancy or birth were less likely to have obtained a diploma or GED than those who had not. For example, at the end of the 42-month study period, 52.7 percent of the controls who had avoided a post-baseline birth, compared with only 37.0 percent of those who had had another baby, had obtained their education credential.

Table 6.7 also shows, however, that women in the experimental group were more likely than those in the control group to have obtained their GEDs or diplomas, regardless of whether or not they had had a post-baseline pregnancy or birth. For example, among the women who had had a post-baseline birth by the 42-month interview, 45.0 percent of the experimentals compared with 37.0 percent of the controls had received a GED or diploma. Among those who had *not* had a post-baseline birth, 60.0 percent of the experimentals and 52.7 percent of the controls had obtained a GED or diploma.[15] Overall, then, the evidence suggests that the short-term negative impact on pregnancies did not adversely affect the program's impact on receipt of a GED certificate. Conversely, however, higher rates of GED/diploma receipt among the experimentals did not translate to an experimental advantage with respect to fertility.

2. Fertility and Living Arrangements. As has been discussed, New Chance had impacts on sample members' living arrangements, including a short-term effect on living with a partner or

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[15] The results shown in Table 6.7 do not reflect experimental impacts, but rather are descriptive experimental/control group differences for subgroups of women with different fertility histories. When outcomes that simultaneously conjoin fertility outcomes with educational attainment are created, however, the positive program impacts are statistically significant; that is, the program significantly increased receipt of an educational credential regardless of pregnancy or birth status (not shown).

Table 6.7

Relationship Between New Chance Sample Members' Receipt of a High School Diploma or GED Certificate and a Post-Baseline Pregnancy or Birth at 18 and 42 Months After Random Assignment

	Experimentals		Controls	
Outcome	Sample size	%	Sample size	%
18 months after random assignment, percentage with diploma/GED among:				
Women who had a post-baseline pregnancy	781	36.9	357	26.1
Women who did not have a post-baseline pregnancy	620	50.0	321	36.3
Women who had a post-baseline birth	340	34.1	160	22.5
Women who did not have a post-baseline birth	1,061	45.4	518	33.7
42 months after random assignment, percentage with diploma/GED among:				
Women who had a post-baseline pregnancy	1,052	48.5	495	39.9
Women who did not have a post-baseline pregnancy	349	61.8	183	55.3
Women who had a post-baseline birth	764	45.0	377	37.0
Women who did not have a post-baseline birth	637	60.0	301	52.7

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including New Chance enrollees (i.e., experimentals) who did not participate in the program.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

husband. This impact could have contributed to the program's short-term effect on increasing post-baseline pregnancies.

Table 6.8 shows that, as might be expected, living with a partner or husband increased the likelihood of a pregnancy for both groups at both follow-up interviews. For example, at 18 months after random assignment, 59.8 percent of the controls who were living with a male partner, compared with 51.5 percent of those who were not, had had a subsequent pregnancy.[16] Living with a partner at the 42-month follow-up was also associated with a higher rate of post-baseline births (not shown in table) among experimentals (59.8 percent) than among controls (51.6 percent).

The table also shows that among the women who were living with a partner, those in the experimental group were somewhat more likely than those in the control group to have gotten pregnant. For example, at the 42-month point, 80.8 percent of the experimentals and 72.4 percent of the controls who were married or cohabitating had had a post-baseline pregnancy.[17] Among women *not* living with a partner, those in the experimental group had only slightly higher post-baseline pregnancy rates than those in the control group at both follow-up interviews. Thus, the findings suggest that there was a somewhat greater likelihood among experimentals that living with a partner would result in a pregnancy—and, at the 18-month point, women in the experimental group were more likely than their counterparts in the control group to be living with a partner.

F. Fertility Outcomes in the New Chance Study and in Other Demonstrations

In the New Chance sample, the rate of pregnancies occurring after random assignment was high for both experimentals and controls, with a significantly higher rate among the experimentals at several points during the follow-up period. Although the findings with regard to post-baseline pregnancy are inconsistent with the intent of the New Chance model, they are not inconsistent with findings from other research. Most programs for young mothers have found that it is extremely difficult to reduce the rate of subsequent pregnancies and births among young women who have given birth as teenagers.

Some specific comparative information is shown in Table 6.9. This table presents rates of post-baseline pregnancy and birth from the evaluations of three other large-scale programs that served disadvantaged young mothers: JOBSTART, the Teenage Parent Demonstration (TPD), and Project Redirection. As is noted in previous chapters, these interventions served somewhat different populations than the young women in New Chance, but comparison data are presented for the most

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[16] Living with a partner at the 18-month interview was unrelated to the initiation of a pregnancy *after* that interview ($r = -.01$), however, perhaps because many of the 18-month living arrangements were not permanent, and perhaps because many of the women living with a man at the first interview had become pregnant prior to the 18-month interview.

[17] Again, the results shown in Table 6.8 are not experimental impacts. When new outcomes that conjoin fertility and living with a partner/husband are created, however, there are significant program impacts. At the 18-month point, significantly more experimentals than controls were living with a partner and had had a post-baseline pregnancy. At the 42-month point, significantly fewer experimentals than controls were living with a partner without having had a post-baseline pregnancy (not shown).

Table 6.8

Relationship Between New Chance Sample Members' Post-Baseline Pregnancy Rate and Living with a Husband or Partner at 18 and 42 Months After Random Assignment

	Experimentals		Controls	
Outcome	Sample size	%	Sample size	%
18 months after random assignment, percentage with a post-baseline pregnancy among:				
Women living with a partner/husband	302	63.2	114	59.8
Women not living with a partner/husband	1,009	55.0	514	51.5
42 months after random assignment, percentage with a post-baseline pregnancy among:				
Women living with a partner/husband	430	80.8	215	72.4
Women not living with a partner/husband	971	72.9	463	72.6

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data and 1,939 sample members for whom there were 18 months of follow up survey data on post-baseline pregnancies and living arrangements, including New Chance enrollees (i.e., experimentals) who did not participate in the program.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

Table 6.9

Fertility-Related Impacts in New Chance and Selected Other Programs

	Post-Baseline Pregnancy			Post-Baseline Birth		
Program and Follow-Up Period	Experimentals (%)	Controls (%)	Difference	Experimentals (%)	Controls (%)	Difference
New Chance						
12 months	43.5	40.7	2.9	11.4	10.7	0.7
18 months	55.9	52.3	3.6	24.4	23.3	1.1
24 months	62.4	59.2	3.2	35.0	33.6	1.4
42 months	75.2	72.8	2.3	54.7	55.5	-0.7
JOBSTART mothers[a]						
24 months	59.1	53.1	6.0	32.7	25.4	7.3 *
Teenage Parent Demonstration						
18 months[b]	48.4	45.6	2.8	27.8	26.3	1.5
24 months[c]	57.6	54.4	3.2	40.1	37.5	2.6
Project Redirection (dropout subsample)						
12 months[d,e]	23.0	34.0 [f]	-9.0 **	N/A	N/A	N/A
24 months[e]	56.0	58.0 [f]	-2.0	32.0	41.0 [f]	-9.0

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data; Cave and Doolittle, 1991; Maynard, Nicholson, and Rangarajan, 1993; Polit, Kahn, and Stevens, 1985.

NOTES: N/A indicates that the specified data item was not available.

A two-tailed t-test was applied to each regression-adjusted difference between experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[a] The JOBSTART sample is made up of young mothers between the ages of 17 and 21 who did not have a high school diploma or GED, read below the eighth-grade level, lived with their own children, and were not enrolled in school at the time of sample enrollment.

[b] The aggregate rates presented here were calculated using information on site-specific rates in the three demonstration sites; significance levels for the pooled sample are not available. At 18 months after random assignment, however, the experimentals had significantly higher rates of pregnancy and birth in one site (Chicago). The 18-month rate in the Teenage Parent Demonstration sample possibly is lower than that in the New Chance sample because of differences in the eligibility criteria for the two studies; in particular, 12 percent of the Teenage Parent Demonstration sample members were pregnant at random assignment.

[c] The aggregate rates presented here were calculated using information on site-specific rates in the three demonstration sites; significance levels for the pooled sample are not available. At 24 months after random assignment, however, the experimentals had a significantly higher rate of pregnancy in two sites (Chicago and Newark) and a significantly higher rate of birth in one site (Chicago).

[d] The Project Redirection dropout subsample is made up of young mothers aged 17 or younger, most of whom were on welfare, who had dropped out of school.

[e] About 60 percent of the Project Redirection sample members were pregnant at baseline. This factor would depress the rates of subsequent pregnancy at follow-up.

[f] In Project Redirection, unlike the other demonstrations, the comparison group was not selected through random assignment.

relevant subgroups of the study samples in these evaluations (except in the case of the Teenage Parent Demonstration, for which comparable fertility data were available for the aggregate sample only[18]).

Two things are clear from the information shown in Table 6.9. First, a majority of disadvantaged young mothers in all evaluations had a subsequent pregnancy within two years of entering the studies, and a sizable minority had a subsequent birth.[19] At 24 months after baseline, the percentages with a subsequent pregnancy spanned a fairly narrow range, from 53.1 percent for JOBSTART controls to 62.4 percent for New Chance experimentals. Second, in the long run none of these programs effectively reduced subsequent pregnancies or births. In Project Redirection there was a favorable impact on subsequent pregnancy at 12 months after baseline, but the difference was not significant at the 24-month point. Moreover, by five years after baseline, those in the experimental group had a significantly higher average number of births than those in the comparison group.[20] A significantly higher birth rate was also observed among the experimentals in JOBSTART at 24 months after random assignment. Births were also significantly higher among the experimentals than among the controls in one of the three Teenage Parent Demonstration sites at both 18 and 24 months after random assignment, and pregnancies were higher among experimentals in two sites at the 24-month point (not shown in the table). Thus, closely spaced pregnancies and births are frequent among poor young mothers; programs have generally been unsuccessful in getting young mothers to postpone subsequent fertility, and unintended program effects are not uncommon.

In the New Chance sample, pregnancies and births during the follow-up period occurred at a high rate regardless of the young women's initial characteristics. The rate of post-baseline births by the end of the study period approached or exceeded 50 percent for every subgroup in both research groups, regardless of the women's initial abilities, family circumstances, or personal resources (see Table 6.6).[21] At the site level, the rate of post-baseline birth was 40 percent or higher among experimental group women in every site by the end of the 42 months, and in some sites the post-baseline birth rate exceeded 70 percent. The consistently high rates of subsequent pregnancies and births across subgroups, sites, and demonstrations suggest that sexual and contraceptive behavior is especially difficult for staff in such programs to influence. For young, sexually active women, the task of avoiding a pregnancy typically must be managed on a daily basis, and it is a task that is subject to a wide range of interpersonal pressures over which program staff have no control. Moreover, program staff are not always comfortable or skilled in dealing with family planning issues. Thus there appear to be a number of reasons—programmatic and other—why programs have difficulty in lowering the rate of subsequent pregnancy among teenage mothers.

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[18] Information on the pregnancy and birth rates was not available for TPD school dropouts who did not have a diploma or GED. The mean number of pregnancies at the time of the follow-up interview, however, was similar for the overall TPD sample and for the dropout subgroup; for example, among experimentals the mean number of pregnancies was 1.0 for the dropout subgroup, compared with .98 for the overall sample, and among controls the means were .99 and .96, respectively.

[19] In a recent report summarizing the results of a three-year follow-up of the LEAP sample (Long et al., 1996), it was reported that about 26 percent of the young mothers in both the experimental and control groups had had a repeat birth in the prior year (that is, between about 25 and 36 months after random assignment). The comparable percentage among New Chance women was 21 percent for those in the experimental group and 22 percent for those in the control group. LEAP data are not included in Table 6.9 because information on the cumulative percentage of women with a post-baseline pregnancy at fixed points in time is not available in the LEAP surveys.

[20] A subsample of the Project Redirection sample was followed up at five years after baseline. In this subsample, the total number of births was 2.4 for the experimentals and 2.0 for the comparison group members ($p < .05$).

[21] Indeed, in a regression analysis attempting to predict the occurrence of a post-baseline pregnancy by 42 months after random assignment on the basis of baseline characteristics, the power of the prediction was extremely low ($R^2 = .06$). The best predictor was the mother's age; those age 20 to 22 at baseline were less likely to have a post-baseline pregnancy than those who were younger.

It is more difficult to find reasons why programs have been found to *increase* the rate of subsequent pregnancy or birth in this population. The New Chance results suggest that the factors affecting contraceptive vigilance, pregnancy planning, and pregnancy termination decisions in this population are complex and that different forces may be operating on different sets of women. Some of the experimental group women may have felt frustrated because their circumstances had not changed as much as they had hoped when they enrolled in New Chance, or they may have been stressed because transitions to new environments such as the workplace or college may have been intimidating. Among these women, there may have been considerable ambivalence about another pregnancy—ambivalence that could have led to inconsistent contraception and to a variety of decisions regarding the resolution of the pregnancy.

Another possibility is that young mothers in self-sufficiency-oriented programs may feel that it is easier to have another baby—an area where many feel that they are successful—than to pursue activities where success may be harder to achieve. The impacts on cumulative post-baseline pregnancy rates in the early months after random assignment could also partially reflect the fact that some young women who attained their GEDs—which was more often the case among the experimentals, especially at the 18-month point—felt that it was advantageous to complete their childbearing before moving on to further schooling or employment. In the New Chance sample, the fact that experimentals who were living with partners were especially likely to become pregnant is consistent with the interpretation that some of the young women felt that the circumstances were right to have another baby.

In summary, it is clear that programs for young mothers face a very stiff challenge in helping young mothers postpone pregnancies—a goal for many programs because of the implications of childbearing for self-sufficiency. Chapter 7 examines the relationship between fertility patterns and economic outcomes such as employment and welfare receipt.

IV. Health-Related Outcomes

The link between poverty and health problems is well documented (see, for example, Gladstein, Rusonis, and Heald, 1992; Hughes et al., 1989), and therefore program developers and operators were concerned with health and access to health care among the young women in the New Chance sample. The program sought to improve the health of program participants and their children through health education classes and linkages to community health facilities; in a few program sites, health-care services were available on site. The health component was designed to promote positive health practices and to address specific health problems that might interfere with regular program participation. This section examines program impacts on health-related outcomes measured in the 18-month and 42-month surveys.[22]

A. Aggregate Program Impacts on Health Outcomes

Table 6.10 summarizes the program effects on health outcomes. With one notable exception, the experimental group and the control group had comparable health-related outcomes at both the 18-month and 42-month interviews. The women in the two groups were equally likely to rate their health as very good or excellent. Experimentals and controls had comparable rates of drinking, drug use, and smoking. Health care coverage was also similar for the two groups at both follow-up points. There was just one program effect that was statistically significant ($p = 0.079$); women in the experimental group reported more non-childbirth hospitalizations during the second part of the follow-up period (Months 19–42) than their counterparts in the control group (19.7 percent versus 16.4 percent). When data from the two follow-up surveys are combined, 24.4 percent of the experimentals and 20.3 percent of the controls reported a non-childbirth hospitalization since baseline ($p < .05$).

B. Subgroup and Site Impacts on Health Outcomes

The program effect on having been hospitalized between the 18-month and 42-month surveys was observed for many subgroups, but there was no consistent subgroup story. In some cases the impact was significant for the more disadvantaged subgroups (for example, those at high risk of depression at baseline, those with no work experience, and those with low reading scores), but in other cases the impacts were found for less disadvantaged subgroups. For example, among the minority of women who entered the study with a high school diploma or GED, 19.7 percent of the experimentals but only 5.1 percent of the controls had a hospitalization (not shown).

In a comparison of the program effect on hospitalizations across the sites, this negative impact was statistically significant in only two sites (Detroit and Pittsburgh); however, there was a modest but statistically significant *favorable* impact in Minneapolis. (The difference in impacts across the sites was statistically significant, $p = .099$.) The negative impact in Detroit was substantial; 28.5 percent of the experimentals and 10.9 percent of the controls at that site were hospitalized between the two survey interviews.

V. Emotional Well-Being

Economically disadvantaged women, including poor young mothers, have been found to suffer from various emotional problems, such as poor self-esteem, high levels of depression and

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[22] It should be noted that the health measures used in the survey are limited and may not adequately capture the full range of issues covered in health education classes. (The survey included several standard and quite general health measures, which in some cases are not linked to specific areas covered in the program.) Moreover, the health outcomes are based on self-reports and do not include objective and sensitive physiologic measures of health status (for example, objective measures of drug use, nutritional status, obesity, and so on).

Table 6.10

Impacts of New Chance on Health Outcomes at or Within 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Personal health rated as very good or excellent (%)				
at 18-month follow-up	54.7	53.2	1.5	0.537
at 42-month follow-up	52.9	51.7	1.2	0.597
Average number of days in bed more than half day due to illness or injury[b]				
RA to 18-month follow-up	5.6	4.2	1.4	0.268
19-month to 42-month follow-up	4.1	3.7	0.4	0.676
Hospitalized at least once[b] (%)				
RA to 18-month follow-up	15.8	13.6	2.1	0.383
19-month to 42-month follow-up	19.7	16.4	3.2 *	0.079
RA to 42-month follow-up	24.4	20.3	4.1 **	0.039
Has no Medicaid or private insurance (%)				
at 18-month follow-up	7.3	8.6	-1.4	0.287
at 42-month follow-up	8.7	9.7	-1.1	0.416
Drank enough alcohol to feel high at least once in prior month (%)				
at 18-month follow-up	37.2	38.2	-1.0	0.668
at 42-month follow-up	37.5	36.4	1.1	0.635
Used drugs at least once in prior month[c] (%)				
at 18-month follow-up	14.3	13.3	1.1	0.517
at 42-month follow-up	11.7	9.5	2.3	0.134
Smoked cigarettes at 42-month follow-up (%)	56.3	59.4	-3.1	0.184
Sample size	1,401	678		

SOURCE: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

RA = random assignment.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] These questions were asked of half the research sample, selected at random.

[c] Includes marijuana, cocaine, crack cocaine, heroin, phencyclidine (PCP), and methamphetamine (ice).

stress, inadequate social support and coping skills, and low feelings of self-efficacy (Coletta, 1983; Ensminger, 1995; Ketterlinus et al., 1991; Musick, 1991). In recognition of the fact that emotional problems can undermine program participation and serve as a barrier to effective functioning in adult roles, New Chance programs were specifically structured to foster positive emotional growth among the participants. Staff deliberately sought to bolster participants' self-esteem and to offer them a warm and supportive but demanding environment. This section reviews the evidence with regard to the program's effects on the emotional well-being of young mothers.

A. Aggregate Program Impacts on Emotional Well-Being

As is indicated in Chapter 3, the women in the New Chance sample had consistently high levels of depression as measured with the Center for Epidemiological Studies Depression (CES-D) scale (see Chapter 3 for details on this scale). At baseline, over half the sample (53 percent) had CES-D scores indicating that they were at risk of clinical depression. And although depression scores declined over time, about 43 percent of the sample were still at risk at the 42-month follow-up.[23]

Because the New Chance program strived to foster emotional well-being among participants, positive effects in this area were expected. Impacts on indicators of emotional well-being are presented in Table 6.11. The table shows that the two groups had comparable scores at baseline, with differences between the two groups increasing over time. By the time of the 42-month interview, the women in the experimental group had significantly higher CES-D scores than those in the control group (means of 16.1 and 15.2, respectively). This difference reflects the fact that while scores in both groups improved (that is, dropped) over time, scores in the control group improved significantly more than scores in the experimental group.[24]

At both follow-up interviews, nearly half the women in both groups obtained a score of 16 or higher on the CES-D. Despite the experimental/control difference in the CES-D score at the final interview, the experimentals were not at statistically significantly greater risk of clinical depression than controls.[25] A comparable percentage of women in the two groups (about 20 percent) were chronically depressed—that is, had scores above 16 at baseline, the 18-month interview, and the 42-month interview.

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[23] Although there was, for the most part, considerable variation in depression scores over time (changes from baseline to the 42-month interview ranged from +47 to – 47 points on a 60-point scale), the best baseline predictor of 42-month CES-D scores was baseline CES-D scores ($r = .32$).

[24] CES-D items were factor analyzed, because other researchers have found that the scale involves several distinct dimensions (Clark et al., 1981; Roberts, 1980). In the New Chance sample, there were three dimensions: (1) a 13-item depression/psychosomatic complaint dimension; (2) a 4-item happiness/positive outlook dimension; and (3) a 2-item perceived social rejection dimension. Women in the experimental group had significantly higher—that is, less favorable—scores than those in the control group on the first dimension only ($p = .029$, not shown).

[25] At the 42-month interview, 22 percent of the experimentals and 20 percent of the controls had scores of 24 or higher on the CES-D, the cutoff for being "at high risk" of depression; this group difference also was not significant.

Table 6.11

Impacts of New Chance on Emotional Well-Being at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Average score on CES-D (depression) scale[b]				
at baseline	17.9	18.7	-0.8	
at 18-month follow-up	16.1	15.7	0.4	0.436
at 42-month follow-up	16.1	15.2	0.9 *	0.082
Change in CES-D score baseline to 42-month follow-up[c]	1.9	3.2	-1.2 **	0.018
At risk of clinical depression (%)[b]				
at baseline	52.2	55.9	-3.7	
at 18-month follow-up	44.4	44.5	-0.1	0.960
at 42-month follow-up	44.6	42.5	2.1	0.355
Chronically depressed (at risk all 3 times) (%)	20.7	20.1	0.7	0.705
Felt stressed much or all of the time in past month, at 42-month follow-up (%)	39.4	33.2	6.2 ***	0.006
Average score on Difficult Life Circumstances (DLC) scale[d]				
at 18-month follow-up	2.7	2.6	0.1	0.256
at 42-month follow-up	2.6	2.4	0.1	0.125
Average score on Mastery Scale[e]				
at 18-month follow-up	22.0	22.1	0.0	0.819
at 42-month follow-up	22.1	22.2	0.0	0.901
Reported no one available as a social support (%)				
at 18-month follow-up	5.5	8.1	-2.6 **	0.031
at 42-month follow-up	5.6	6.0	-0.4	0.693
Average level of satisfaction with available social support (%)[f]				
at 18-month follow-up	8.2	7.9	0.3 **	0.013
at 42-month follow-up	8.5	8.5	0.0	0.822
Average score on Social Support Scale at 42-month follow-up	21.3	21.3	0.0	0.906
Satisfied/very satisfied with standard of living at 42-month follow-up (%)	69.7	73.7	-4.0 *	0.062
Sample size	1,401	678		

(continued)

Table 6.11 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60. Those with scores below 16 on the CES-D are considered not to be at risk of depression; those with scores of 16 and above are considered at risk.

[c] The values here reflect baseline CES-D scores minus follow-up CES-D scores. Positive values indicate improvement—i.e., less depression at follow-up.

[d] The scores are the total numbers of ongoing problems or stresses the respondent faces, of a list of 10 problems.

[e] The Mastery Scale measures sense of mastery over personal events. Scores can range from 7 to 28.

[f] Satisfaction with social support was rated on a scale from 0 ("extremely dissatisfied") to 10 ("extremely satisfied").

In the 42-month survey, women were also asked how much of the time in the previous month they felt highly stressed. More experimentals (39.4 percent) than controls (33.2 percent) reported feeling stressed all or much of the time in the prior month. The difference was statistically significant.[26]

Both follow-up interviews included a measure of ongoing stress, the Difficult Life Circumstances (DLC) scale. Unlike many stress scales, which focus on life changes over a fixed period of time, the DLC scale was designed to measure the ongoing or habitual stress that is often a feature of living in disadvantaged circumstances. The 10-item scale used in this study was adapted from a scale by Booth and colleagues (1989).[27] As is shown in Table 6.11, the average number of reported difficult life circumstances declined somewhat between the 18-month and 42-month interviews, from an average of 2.7 to 2.6 for the experimental group and from 2.6 to 2.4 for the control group. Group differences were not significant at either point, although at the final interview the group difference favoring controls was almost statistically significant ($p = .125$).[28]

Another measure of emotional well-being used in the two follow-up surveys was the Mastery Scale (Pearlin et al., 1981), a seven-item scale designed to measure a person's sense of self-efficacy or mastery over external events.[29] Scores on this scale can theoretically range from 7 (low perceived self-efficacy) to 28 (high self-efficacy). For both the experimentals and controls, the mean Mastery Scale score was 22 at both follow-up interviews, suggesting similar (and fairly positive) perceptions of self-efficacy.

Several brief measures of social support were also included in the follow-up interviews. Respondents were asked to whom they could turn for moral support when they had a problem or just needed to talk.[30] At the 18-month interview, 5.5 percent of the women in the experimental group, compared with 8.1 percent of the controls, said they had *no one* available as a social support, a difference that was statistically significant. By the time of the 42-month survey, the percentage of controls with no social support declined, so that group differences were no longer statistically significant. Respondents also rated their degree of satisfaction with the social support available to them on a scale from 0 ("extremely dissatisfied") to 10 ("extremely satisfied"). As is indicated in Table 6.11, women in the experimental group gave significantly higher average ratings of satisfaction with available social support than those in the control group at the 18-month point (8.2 versus 7.9, respectively), but the ratings were comparable at the final interview.

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[26] There was a strong correlation ($r = 0.46$, $p < 0.01$) between the CES-D score and stress reported at the 42-month interview.

[27] Respondents were asked whether they were experiencing 10 specific problems. Examples of items are "Do you have a relative or boyfriend who is in jail?" and "Have you been robbed, mugged, or attacked in the past year?" Total scores, calculated by summing the number of "yes" responses, ranged from 0 to 9 in this sample at both follow-up interviews. Standard internal consistency reliabilities were not considered appropriate for this scale, because there is no reason to expect that a person with one particular problem would also have another particular problem.

[28] About 90 percent of the sample reported 1 or more of the 10 listed problems at both interviews. The most commonly reported problem, cited by over 40 percent of the sample at both follow-up interviews, was trouble finding a good place to live. Scores on the DLC scale were significantly correlated with CES-D scores ($r = .39$) and with responses to the question on feeling highly stressed ($r = .30$).

[29] An example of an item from the Mastery Scale is "I have little control over the things that happen to me." In the present study, the internal consistency reliability of this scale was .70 at the 18-month interview and .71 at the 42-month interview. Actual scores spanned the full theoretical range, from a low of 7 to a high of 28, at both interviews.

[30] On average, women in both groups cited close to three different types of people whom they could count on for support. Mothers were most frequently mentioned at both interviews (47 percent at 18 months and 45 percent at 42 months), followed by female friends (39 percent at both surveys) and partners (35 percent at 18 months, 31 percent at 42 months).

The 42-month survey also included a seven-item Social Support Scale that was developed specially for the New Chance study.[31] The average score on this scale was 21.3 in both groups. This score is above the neutral midpoint on the scale (17.5), and therefore suggests reasonably positive perceptions of social support.

Finally, women were asked at the final interview how satisfied they were with their standard of living.[32] A lower percentage of women in the experimental group (69.7 percent) than in the control group (73.7 percent) reported that they were satisfied with their standard of living. This difference was statistically significant.

Thus, the effects of New Chance on indicators of emotional well-being appear to be mixed but were mostly unfavorable. In the short term, New Chance appears to have had a positive effect on the young women's social support, but by the end of the study period, women in the experimental group felt more highly stressed, were more depressed, and were less satisfied with their standard of living than women in the control group.

B. Subgroup and Site Impacts on Emotional Well-Being

Table 6.12 shows that, for most subgroups, depression scores had improved less between baseline and the final interview for women in the experimental group than for their counterparts in the control group. The program effect was especially large among women who had been out of school for two years or more at random assignment—controls in that subgroup improved their CES-D scores by 3.5 points, compared with about 1 point for experimentals. The difference across subgroup impacts for the subgroups based on length of time out of school was statistically significant. Impacts on improvements to CES-D scores over the 42 months of follow-up were statistically significant for several other subgroups, including women who entered the study at high risk of depression and women whose families were always on welfare when these women were children.

The program caused an increase in feelings of stress measured at the 42-month interview for most subgroups (not shown). An especially large program effect was observed for women whose families had always been on welfare when they were young (40.4 percent of experimentals and

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[31] An example of an item from the Social Support Scale is "When I'm feeling worn out or sad, I have to deal with it alone." The internal consistency reliability for this scale was .75. Scores could range from 7 (absence of social support) to 28 (abundant social support); actual scores ranged from 8 to 28.

[32] The question was asked as follows: "Could you please tell me how you feel about your standard of living now—your housing, medical care, furniture, clothing, recreation, and things like that? Would you say you are very satisfied, /satisfied, dissatisfied, or very dissatisfied with your standard of living?"

Table 6.12

Impacts of New Chance on Changes in CES-D Scores from Random Assignment to 42-Month Follow-up, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Change in CES-D Score[a]		Within-Subgroup Impact	p[b]	Difference Across Subgroup Impacts[c]	p[b]
		Experimentals	Controls				
Age (years)						---	0.150
16-17	402	1.9	1.4	0.5	0.681		
18-19	997	2.3	3.3	-1.1	0.149		
20-22	678	1.5	4.0	-2.4 ***	0.008		
Ethnicity						---	0.425
Black, non-Hispanic	1,087	1.4	3.1	-1.7 **	0.016		
Hispanic	474	3.5	3.5	0.0	0.999		
White or other	515	1.6	3.0	-1.4	0.213		
Highest grade completed						1.6	0.142
10th or below	1,391	2.0	2.7	-0.7	0.267		
11th or above	684	1.8	4.2	-2.3 ***	0.010		
Interval since last attended regular high school						-2.6 **	0.014
More than 2 years	1,093	1.0	3.5	-2.5 ***	0.001		
2 years or less	927	2.9	2.8	0.1	0.896		
TABE reading test score (grade equivalent)[d]						---	0.457
Below 6th grade	433	1.2	0.8	0.4	0.748		
6th or 7th grade	492	0.8	2.5	-1.7	0.109		
8th or 9th grade	566	2.4	3.8	-1.4	0.163		
10th grade or above	583	3.1	5.0	-1.9 *	0.058		
Family received AFDC when sample member was growing up						---	0.492
Always	341	1.5	3.8	-2.3 *	0.074		
Sometimes	970	2.2	3.5	-1.4 *	0.066		
Never	749	1.9	2.4	-0.5	0.566		
Ever employed						0.8	0.504
Yes	1,646	1.9	3.3	-1.4 **	0.019		
No	433	2.3	2.8	-0.5	0.642		
CES-D (depression) Scale						---	0.629
0-15 (not at risk)	967	-4.2	-3.3	-0.9	0.253		
16-23 (at some risk)	525	3.2	4.1	-0.9	0.367		
24-60 (at high risk)	582	10.9	12.8	-1.9 **	0.038		
Multiple risk score[e]						---	0.287
Low	871	2.4	3.1	-0.8	0.352		
Moderate	618	1.6	2.6	-1.0	0.295		
High	525	1.5	4.2	-2.8 ***	0.009		

(continued)

Table 6.12 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a]The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60. The values here reflect baseline CES-D scores minus the follow-up scores. Positive values indicate improvement—i.e., less depression at follow-up.

[b]A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as ***= 1 percent; **= 5 percent; *= 10 percent.

[c] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[d]The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[e]To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

29.4 percent of controls in this subgroup reported higher feelings of stress at 42 months than at baseline) and among women who were at high risk of clinical depression at baseline (50.5 percent of experimentals and 41.5 percent of controls)

There were no statistically significant program effects on the 42-month CES-D depression scores at any site. With respect to improvements to CES-D scores over the 42 months of follow-up, there were statistically significant impacts favoring controls in two sites (Minneapolis and Portland), and a significant impact favoring experimentals in one site (Salem); however, the difference across subgroup impacts was not statistically significant. More experimentals than controls in Minneapolis and San Jose reported feeling highly stressed at the 42-month interview, but here again the between-sites test was not statistically significant.

C. Perspectives on the Emotional Well-Being Impacts

The impact findings suggest that the New Chance program experience had some negative long-term effects on the emotional well-being of young mothers. Three and a half years after random assignment, women in the experimental group had higher levels of depression, felt more highly stressed, and were less satisfied with their standard of living than women in the control group. Moreover, many of the unintended negative program impacts were especially pronounced among women who already were at high risk of clinical depression at baseline. In this subgroup women in the experimental group were more likely than their control group counterparts to have moved many times, to have had trouble finding a good place to live, to have been hospitalized for a non-childbirth reason, to have high scores on the Difficult Life Circumstances scale, to have smaller improvements on the depression scale, to feel highly stressed, and to be dissatisfied with their standard of living. (As is discussed in Chapter 7, there is some evidence that their actual standard of living may have been somewhat lower as well.)

These effects are puzzling and disturbing, given the program's goals to enhance enrollees' self-esteem and their feelings of personal control. Additional analyses were undertaken in an effort to shed light on how these unintended effects could have occurred.

1. **Depression at the Final Interview in Relation to Other Outcomes.** At the time of random assignment, the young women in the sample who were most highly depressed also had a number of other barriers to overcome. For example, those sample members who had baseline CES-D scores above 23 (that is, those considered in the high-risk category) tended to have been out of school for two or more years, to have low reading scores, to have low self-esteem, and to have been on welfare during their childhood. In interpreting the depression findings at the 42-month survey, then, it is important to consider other outcomes in relation to depression, because depression and emotional problems occur within the context of other life circumstances.

Table 6.13 presents information on various 42-month outcomes for three subgroups of women in the New Chance sample (experimentals and controls combined) based on 42-month CES-D scores: those not at risk of depression (CES-D scores of less than 16), those at some risk of depression (scores of 16–23), and those at high risk of depression (scores of 24 or higher). Consistent with patterns observed at baseline, women in the group with the highest depression

Table 6.13

Selected 42-Month Outcomes for New Chance Sample Members by Depression Risk at 42 Months After Random Assignment

Outcome	Not at Risk of Depression at 42 Months (CES-D < 16)	At Risk of Depression at 42 Months (CES-D 16-23)	At High Risk of Depression at 42 Months (CES-D > 23)
Received diploma/GED (%)	57.1	42.6	40.5
Received college credits (%)	18.6	10.2	6.8
Living with a parent/grandparent (%)	19.9	22.9	22.4
Living with a partner/husband (%)	33.1	31.1	23.9
Living with kids only (%)	36.7	36.3	39.8
Living in another arrangement (%)	10.2	9.8	13.9
Living with none of her children (%)	2.5	2.3	7.8
Living without one of her children (%)	7.9	10.9	18.4
Number of moves since birth of focal child	3.6	3.8	4.3
Ever pregnant since random assignment (%)	71.5	78.7	78.3
Ever gave birth since random assignment (%)	51.6	55.8	62.4
Hospitalized since 18-month interview (%)	15.5	17.7	25.9
Average score on Difficult Life Circumstances scale	2.0	2.8	3.6
Satisfied with standard of living (%)	76.6	69.5	54.9
Employed (%)	32.3	24.5	21.5
Below poverty (%)	72.8	76.6	78.8
On welfare (%)	67.8	76.4	74.4
Earnings since random assignment ($)	7,204	5,753	4,801
Sample size	1,083	441	410

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 1,934 sample members for whom there were 42-month follow-up survey data on selected outcomes including a CES-D score. Calculations included New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable responses from some sample members' questionnaires.

scores were also the most disadvantaged with regard to many other aspects of their lives.[33] For example, educational attainment was strongly related to depression scores. Over half (57.1 percent) of those in the not-at-risk group had a GED or diploma at the final interview, compared with only 40.5 percent in the high-risk group. Similarly, women who were less depressed were more likely to have earned college credits than women with higher levels of depression.

With respect to living arrangements, the percentages of women living with a parent or grandparent or living alone with their children were similar in the three groups. Women who were in the high-risk group, however, were somewhat less likely than other women to be living with a partner or husband at follow-up and were somewhat more likely to be living in an "other" arrangement. Moreover, depression was strongly related to having a child living elsewhere. Nearly one-fifth of the women in the high-risk group (18.4 percent) had at least one child not living with them, compared with only 7.9 percent of those in the not-at-risk group. Depressed women also faced more residential instability; those in the high-risk group had moved an average of 4.3 times since the birth of the focal child, while those in the not-at-risk group had moved an average of 3.6 times.

Fertility outcomes were also related to depression. Nearly 80 percent of the women in both the at-risk and the high-risk groups had had a post-baseline pregnancy, compared with 71.5 percent of the women in the not-at-risk group. Those in the group with the highest follow-up depression scores were also more likely than those in the other two groups to have had a birth in the 42-month follow-up period. Also of note is the strong relationship between depression and hospitalizations that were unrelated to childbirth; 25.9 percent of those in the high-risk group, compared with 15.5 percent of those in the not-at-risk group, had had such a hospitalization between the 18-month and 42-month interviews. (Since information on the cause of hospitalizations was not obtained, it is impossible to know if perhaps some of the hospitalizations were for mental health problems.)

Not surprisingly, highly depressed women cited substantially more problems on the Difficult Life Circumstances scale (mean of 3.6) than nondepressed women (mean of 2.0); the most highly depressed women also were much less satisfied than other women with their standard of living. And there are several indications that the women in the high-risk group actually were worse off financially than women with lower depression scores. As is shown in Table 6.13, the women with high CES-D scores were less likely to be employed, more likely to be on welfare, and more likely to have a projected income below the poverty level than other women—particularly women in the not-at-risk group. The total average earnings during follow-up of the women with low depression scores was 50 percent higher (mean of $7,204) than those of women with the highest depression scores (mean of $4,801).

In summary, the women in the New Chance sample who had the highest depression scores at follow-up were the women who had made the least progress toward self-sufficiency; they were women who typically had not obtained a diploma or GED , had had another baby, were living alone with their children, and were unemployed. They also appeared to be a group of women who were especially vulnerable to other serious problems, such as a major health condition requiring hospitalization and circumstances that led to a child's living elsewhere. Again, the direction of influence cannot be determined from these descriptive analyses. Women who were suffering from emotional problems may have had an especially hard time finishing school or getting a job, for example. But repeated failure and disappointments across a wide range of experiences may also have led to depression.

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[33] Note that this table does not contain any information on the possible causal direction of the relationships shown. That is, while depression may affect other outcomes, the reverse could also be driving apparent relationships.

2. **The Frustrated-Expectations Hypothesis**. Although the findings just described shed some light on the factors that might contribute to the high overall levels of depression in the New Chance sample, they do not provide an understanding of why the *impacts* on emotional well-being were negative—that is, why the women in the experimental group had more stress and depression at the final interview than those in the control group. Three possible explanations for these impacts are explored, one of which relates to children's outcomes and is discussed in Chapter 8. This section focuses on an explanation that concerns a possible conflict between the program's encouragement and emphasis on self-sufficiency on the one hand and the actual circumstances of the young women at the 42-month point on the other.

New Chance was more than just a GED-attainment program; it was a program that professed to offer young mothers a "new chance" to move forward with their lives, to become empowered, and to develop skills that would put them on a path toward successful adult roles. It is possible that the program message, philosophy, and goals were unrealistic for some of these young women, especially those who came into the program with an array of deep-rooted problems and barriers to self-sufficiency. According to this hypothesis, the program may have created expectations for improved life circumstances that, when not attained, created more stress and depression than the women would have experienced in the absence of the program. In essence, the program may have had a negative effect on emotional well-being because it raised hopes but ultimately created another experience of failure for these women.

If this frustrated-expectations hypothesis has any validity, it apparently is not related to the young women's expectations regarding educational attainment. As is discussed in Chapter 5, women in the experimental group were significantly more likely than those in the control group to have obtained a diploma or GED by the end of the study, and this impact was observed across virtually every subgroup examined. Moreover, as Table 6.13 shows, women who completed their basic schooling were less likely to have high depression scores than those who did not. These findings would lead one to expect lower rather than higher depression scores among experimentals.[34]

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[34] Even among women who were at high risk of depression at the end of the study (that is, those in the third column of Table 6.13), experimentals were more likely than controls to have obtained a diploma or GED (42.3 percent versus 31.2 percent, respectively). The correlation between having a diploma or GED and CES-D scores was –.16 and was similar in magnitude for both research groups; this finding suggests that experimentals without a GED were not more depressed than controls without a GED as a result of greater disappointment with their educational attainment.

Women in the experimental group who managed to obtain a GED certificate, however, may have been subject to stress and frustration if having a GED did not result in improving their financial circumstances. As will be discussed in Chapter 7, at the end of the study the two research groups had similar economic outcomes—and yet, as has been discussed (see Table 6.11), the women in the experimental group were significantly less satisfied than women in the control group with their standard of living.

Table 6.14 presents some information that lends support to the frustrated-expectations hypothesis. This table presents CES-D change scores (that is, average improvements to the depression scores from baseline to the 42-month interview) for experimentals and controls in different educational and economic circumstances at the end of the study. Specifically, the employment status, earnings, and welfare receipt of those having versus not having a diploma or GED at the end of the study were compared. This table indicates that average improvements to depression over time were nearly universal across these categories in both research groups (that is, all change scores were positive); improvements tended to be especially great among those who obtained a diploma or GED and who had the most favorable economic circumstances at the final interview. That is, the mean change scores tended to be largest among women in the top row of each of the three panels. In every category in this table, the improvements were greater for controls than for experimentals, regardless of the educational and economic circumstances of the women. Consistent with the frustrated-expectations hypothesis, however, the magnitude of the experimental/control group difference was greatest among those who had obtained a high school diploma or a GED but were in a less desirable economic category. For example, among women who had an educational credential but whose earnings in the year before the 42-month interview were less than $3,000 (second panel), CES-D scores had improved by 1.9 points in the experimental group but by 4.3 points in the control group.[35]

3. **The Disrupted-Living-Arrangements Hypothesis.** An alternative explanation for the impacts on depression and stress concerns the program effects on the living arrangements of participants. As is described earlier in this chapter, New Chance had both short-term and longer-term impacts on living arrangements, impacts that suggest greater residential instability and earlier departures from parental households for experimentals than for controls. It seems plausible that women whose living arrangements were in flux would be more stressed and vulnerable to depression; this conjecture is supported by the evidence in Table 6.13, which indicates that women in the high-risk depression group were especially likely to be living in an irregular arrangement, to have moved many times, and to be living without their own children.

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[35] Since there is abundant evidence that stress and depression on the one hand and poverty and economic adversity on the other are correlated, it was important to determine whether the program's impacts were being driven by group differences in actual financial circumstances rather than by expectations for economic improvements. Although women in the experimental and control groups were fairly similar in terms of most economic outcomes (as will be described in Chapter 7), there were some negative subgroup impacts, and these impacts were especially likely to occur among the most disadvantaged subgroups for whom the impacts of emotional well-being were strongest. Even in analyses that included measures of economic well-being, however, the program's negative effects on depression and satisfaction with standard of living were sustained.

Table 6.14

Improvements to CES-D Scores (from Random Assignment to 42-Month Follow-Up) for New Chance Sample Members, by Economic and Educational Status

Outcome/Subgroup	Sample Size	Experimentals[a]	Controls[a]	Difference[b]
Employment status and educational attainment				
Employed, has a diploma/GED	353	4.1	5.3	-1.2
Not employed, has a diploma/GED	618	1.4	3.8	-2.4 **
Employed, no diploma/GED	192	2.0	3.5	-1.6
Not employed, no diploma/GED	766	1.3	2.0	-0.7
Earnings and educational attainment				
Earned $3,000+ prior year, has a diploma/GED	358	3.4	4.1	-0.7
Earned < $3,000 prior year, has a diploma/GED	613	1.9	4.3	-2.4 **
Earned $3,000+ prior year, no diploma/GED	211	1.2	2.9	-1.7
Not employed, no diploma/GED	747	1.4	2.3	-0.9
Welfare receipt status and educational attainment				
Not on welfare, has a diploma/GED	342	3.8	4.3	-0.5
On welfare, has a diploma/GED	629	1.8	4.1	-2.2 **
Not on welfare, no diploma/GED	214	0.3	2.1	-1.8
On welfare, no diploma/GED	744	1.8	2.3	-0.5

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60. Those with scores below 16 on the CES-D are considered not to be at risk of depression; those with scores of 16 and above are considered at risk.

Calculations for this table used data for 1,929 sample members for whom there were CES-D scores at 42 months and 42-month follow-up survey data on economic and educational status, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] The values here reflect baseline CES-D scores minus follow-up CES-D scores. Positive values indicate improvement—i.e., less depression at follow-up.

[b] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent. Note that differences shown do not represent experimental impacts because subgroups were defined using post-random assignment outcomes.

Table 6.15 shows experimental and control group differences within the three depression categories (not at risk, at some risk, and at high risk) for various outcomes relating to living arrangements and relationships. For most outcomes, the experimental/control differences were small among women who were either not at risk or at moderate risk of depression at the 42-month interview. For example, among nondepressed women, the average number of moves was 3.5 for experimentals and 3.6 for controls. Among the women at high risk of depression, however, the experimental/control group differences tended to be fairly large for those outcomes that suggest less stability in living arrangements—that is, for those living arrangement outcomes for which there was an overall program impact. For example, the average number of moves was 4.7 for experimentals in the high-risk group, compared with 3.6 for controls. As another example, 48.8 percent of the high-risk experimentals, compared with only 41.3 percent of the high-risk controls, said that they had had trouble finding a good place to live in the previous year. Thus, high depression scores appear to be co-occurring with the negative program impacts on living arrangement outcomes.[36]

If the impacts on living arrangements were totally responsible for the impacts on depression, however, one would expect the impacts on depression to disappear when the living arrangement variables were statistically controlled. Regression analyses were performed in which both baseline characteristics and three other living arrangement variables at the time of the 42-month survey (number of moves, living in an "other" arrangement, and having a child who lives elsewhere) were included in the model. The inclusion of these covariates reduced the impact on CES-D change scores from 1.23 points to 1.12 points, but, nevertheless, the group difference remained statistically significant ($p = .029$).

In summary, there are no clear-cut answers as to what factors contributed to impacts on emotional well-being at the 42-month point. Two alternative—and not mutually exclusive—hypotheses for the impacts were briefly explored in a mainly descriptive fashion. The data are consistent with the hypothesis that the women in the experimental group may have been more unhappy with their lives than controls because their higher expectations for improved life circumstances were not met. But the program's effects on living arrangements may also have contributed to the stresses in these women's lives.

It is important to note that both explanations are further supported by the subgroup impact analyses. These analyses suggest that the women who were most at risk of depression when they applied to the program were the ones most likely to experience adverse program impacts on depression and stress, on dissatisfaction with their standard of living, and on living arrangement outcomes. The subgroup of women who were at high risk of depression at baseline were probably the young mothers who were most in need of stable living arrangements but who at the same time were least likely to have had stability in their parental homes and who were thus most subject to intervention in this area. They were also likely to be young women who were most economically

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[36] In general, the relationship between 42-month depression scores and living arrangement variables was much stronger for experimentals than for controls. For example, the correlation between number of moves and CES-D scores was .13 for experimentals ($p < .001$) and .01 for controls. This finding suggests the possibility that the reasons for the moves may have been different for the experimentals and for the controls.

Table 6.15

Relationship Between Living Arrangement Outcomes and Depression Risk for New Chance Sample Members at 42 Months After Random Assignment

	Not at Risk of Depression at 42 Months (CES-D < 16)		At Risk of Depression at 42 Months (CES-D 16-23)		At High Risk of Depression at 42 Months (CES-D > 23)	
Outcome	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Living with a parent/ grandparent (%)	20.2	19.5	24.7	19.3	22.3	22.8
Living with a partner/ husband (%)	33.6	32.0	30.1	33.1	23.0	26.0
Living with children only (%)	35.5	39.3	36.2	36.5	38.2	43.3
Living in another arrangement (%)	10.8	9.2	9.1	11.0	16.6	7.9
Living with none of her children (%)	2.6	2.2	2.4	2.0	9.5	3.9
Living without one of her children (%)	8.3	6.9	11.1	10.3	19.1	16.7
Number of moves since birth of focal child[a]	3.5	3.6	4.0	3.4	4.7	3.6
Hospitalized since 18-month interview (%)	16.2	14.2	18.9	15.2	28.3	20.5
Sample size	724	359	296	145	283	127

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 1,934 sample members for whom there were CES-D scores at 42 months and 42-month follow-up survey data on living arrangements, including New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable responses from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment.

[a] For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

disadvantaged at the outset and who most acutely wanted to improve their financial circumstances but who at the same time had numerous barriers that made economic progress most difficult. In short, the program may have had negative effects on the most needy group of women because the intervention was powerful enough to create hope but not powerful enough to address circumstances that likely were created by years of such problems as family dysfunction, poverty, low cognitive and emotional resources, and academic failure.

Chapter 7

Impacts on Employment, Earnings, Welfare Receipt, and Family Income

I. Introduction

The New Chance program was focused on improving employment outcomes and economic well-being for the young families it served. In targeting young mothers on welfare who were teenagers when they had their first child and who had not completed their high school education, the program aimed to improve the lives of a group that is among the most economically and socially disadvantaged in society. Employment and welfare statistics show that women who become parents in their teens and who begin receiving AFDC at an early age are likely to remain on welfare for a long time (see Bane and Ellwood, 1994; Ellwood, 1986; Pavetti, 1992).

Before entering the New Chance study, sample members had had only limited work experience. While the majority of sample members (78.9 percent) had held a job at some point, only 36.7 percent had worked in the year preceding their application to New Chance. Of those who had worked during that year, 46.2 percent earned less than $500.

The New Chance program was designed and implemented as a human capital development model. The program, therefore, offered extensive education and training services to improve the employment opportunities of the young women who enrolled, rather than trying to move participants into employment soon after entering the program. An expected short-term consequence of such an approach is that intensive participation in education and training may lower employment rates and earnings initially. This is considered the *opportunity cost* of participation in a program like New Chance. By increasing sample members' human capital through education and training, the program aimed to improve employment outcomes in the long run, creating a payoff that was supposed to be large enough to more than offset the initial opportunity cost.

As was discussed in Chapter 4, the service differential between experimentals and controls in New Chance was smaller than program developers had intended. While 89.2 percent of the experimental group participated in education and 47.5 percent received skills training, participation among control group members was substantial as well (76.2 percent and 38.1 percent for education and skills training, respectively). When average weeks of education and training are compared, the incremental effects of New Chance are also limited. The program increased the average number of weeks of education from 26.4 weeks for the control group to 37.0 weeks for the experimental group. It increased the average number of weeks in skills training from 10.1 weeks for controls to 13.4 weeks for experimentals.

These effects on participation were followed by program effects on attainment of education credentials: an increase from 43.8 percent for controls to 51.9 percent for experimentals in the probability of having a high school diploma or a GED at the 42-month interview. The program also increased attainment of a training certificate or trade license from 24.7 percent for controls to 25.2 percent for experimentals. Given that program effects on all these education and training outcomes were found to be smaller than intended, subsequent effects on employment outcomes were expected to be modest also.

A. The Scope of This Chapter

The body of this chapter is divided into three parts, which describe program effects in three different areas. The first is devoted to employment outcomes, including employment rates, weeks and hours worked, and earnings. The second section presents program effects on welfare receipt, and the third section revolves around income sources and income levels in the month prior to the 42-month interview.

The employment and welfare sections present experimental program effects, first for the full sample and then for selected subgroups and for the sixteen New Chance sites. In an effort to better explain the overall experimental findings, the employment and welfare sections supplement the experimental findings with nonexperimental analyses that directly explore relationships between key outcomes. Specifically, in the employment section, results are presented from nonexperimental analyses of the relationship between education outcomes (participation in educational offerings and receipt of education credentials) and employment outcomes. In the welfare section, outcomes are analyzed as they vary over time, and links between welfare receipt and employment outcomes are studied more closely.

B. A Preview of the Findings

Throughout the 42-month follow-up period, the effects of New Chance on employment outcomes, earnings, welfare receipt, and family income were generally small and not statistically significant. The program slightly lowered employment rates, hours worked, and earnings during the first year of follow-up, as anticipated, but these modest upfront reductions in work effort were not followed by the expected positive effects on employment and earnings later on in the follow-up period. As was shown in Chapter 3, however, both experimentals and controls had relatively high levels of employment compared with other samples of welfare recipients and young single parents.

The lack of positive program effects on employment and earnings appears to be related to the amount and mix of services that New Chance participants received. An analysis of the benefits of different services and education credentials suggests that participation in skills training and college were associated with positive employment outcomes. Unfortunately, many New Chance experimentals received only adult education (ABE/GED), which by itself was not found to have positive effects on subsequent employment outcomes.

Between the 18- and 42-month interviews, income levels for both research groups increased substantially. At the end of the follow-up period, however, most sample members continued to rely on welfare for the major portion of their income, and most continued to live in poverty. Many sample members reported having too little income to secure basic necessities like electricity, gas, telephone service, and sometimes even food.

II. Impacts on Employment Outcomes

A. Impacts on Employment Rates

Table 7.1 and Figure 7.1 show the impacts of New Chance on employment rates during the three-and-a-half-year follow-up period. It is interesting to note first the absolute employment levels during this period and their change over time. The figure and the year-by-year employment rates reported in the table show that 38.7 percent of New Chance experimentals worked at some time during the first predominantly "post-program" year (Months 7 through 18[1]), a proportion that increased steadily after that, to 41.4 percent who worked at some point in the following year (Months 19 through 30) and 53.3 percent in the final year (Months 31 through 42).

While employment rates among New Chance experimentals were substantial and comparable to those found in other studies, controls did not lag behind. Table 7.1 shows that controls had higher employment rates than experimentals in the first six months after random assignment (the "in-program" period for the experimental group) and roughly similar patterns of employment in the three years that followed. As Chapter 3 showed, these employment rates exceeded those found in other studies of teen parents and were comparable to those found for larger and more representative samples of welfare recipients.

Thus, aside from a modest but statistically significant reduction in work effort during the first six months after random assignment, New Chance did not affect sample members' employment patterns. When the focus is narrowed to *full-time* employment (that is, 30 hours a week or more), the table shows similar patterns of program effects. The percentage of sample members who were ever employed full-time increased over time, with over half of all sample members reporting any full-time employment during the 42-month follow-up period.

The third panel of Table 7.1 shows outcomes that describe sample members' employment status at the time of the 42-month interview. Less than a third of all sample members were employed at that time (27.8 percent of experimentals, 30.8 percent of controls), leaving more than two thirds either unemployed or not in the labor force. Almost 75 percent of those employed at follow-up were employed full-time (20.6 percent of experimentals and 22.2 percent of controls).

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[1] Over half (58.8 percent) of all experimental group members completed their participation within six months after random assignment. By the end of a year after random assignment, 85.2 percent had completed their participation.

Table 7.1

Impacts of New Chance on Employment Rates at or Within 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Ever employed (%)				
Months 1-6	15.1	20.4	-5.2 ***	0.002
Months 7-18	38.7	39.7	-1.0	0.657
Months 19-30	41.4	39.5	1.9	0.403
Months 31-42	53.3	50.5	2.8	0.213
Months 1-42	69.5	66.2	3.3	0.114
Ever employed full time (30 hours/week or more) (%)				
Months 1-6	8.5	12.7	-4.2 ***	0.002
Months 7-18	24.5	26.7	-2.3	0.252
Months 19-30	31.0	28.9	2.1	0.322
Months 31-42	42.3	40.5	1.8	0.417
Months 1-42	56.1	54.9	1.1	0.611
At 42-month follow-up (%)				
Employed	27.8	30.9	-3.1	0.124
Employed full time (30 hours/week or more)	20.5	21.9	-1.4	0.440
Not employed, but looking for work	28.2	28.0	0.3	0.901
Not in the labor force	43.8	41.1	2.8	0.228
Average number of months until first employment	28.05	27.52	0.53	0.614
Average number of months first employed	6.19	5.98	0.21	0.788
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

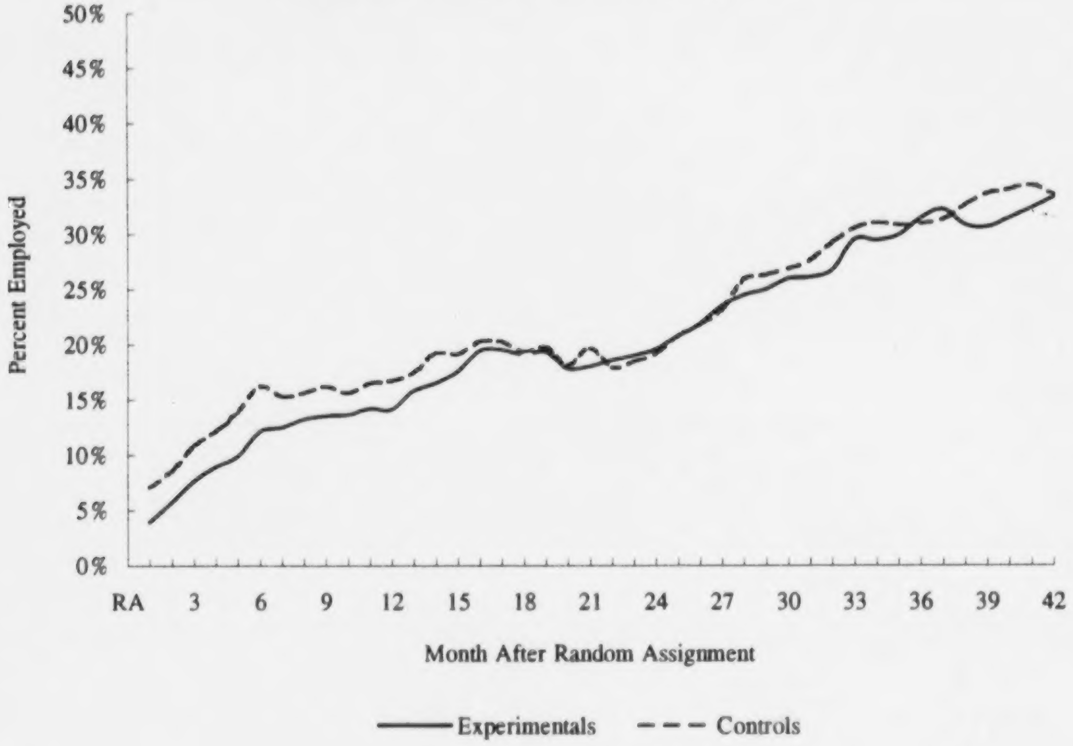
NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Figure 7.1

Monthly Employment Rates for New Chance Sample Group Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.7 for data corresponding to figure.

Of sample members who were not employed at follow-up, about half were actively looking for a job. Consequently, 43.8 percent of experimentals and 41.1 percent of controls were not in the labor force at the time of the 42-month follow-up interview (for reasons to be discussed in the next section). The difference was not statistically significant.

Employment rates and program effects on employment may be broken down into two distinct components, namely (1) *getting* a job and (2) *keeping* a job. Each of these two components could have been affected by New Chance, and each offers a possible explanation for employment effects or the lack thereof. For example, New Chance may have delayed entry into employment, but it may also have helped experimental group members to retain their jobs once they secured them. These two effects would cancel each other out in a simple comparison of employment rates.[2] It appears that New Chance lengthened the average time until the first month of employment by 0.53 months (from an average for the control group of 27.52 months), while simultaneously increasing the average job tenure by 0.21 months (on a control mean of 5.98). Neither one of these estimates was statistically significant.

B. Job Characteristics

Table 7.2 describes selected characteristics of the last (or current) job held by sample members who reported any employment during the 42-month follow-up period.[3] It appears that job characteristics were quite similar across the two research groups. Of employed experimentals, 23.9 percent had sick days, 27.9 percent had paid vacation, and 21.4 percent had a health plan or medical insurance. Among employed controls, these numbers were 24.2, 30.3, and 22.5 percent, respectively. Experimental and control group members were similarly satisfied with their jobs; on a scale of 0 to 10 (with 10 being most favorable), experimentals gave their current or most recent jobs a rating of 6.2 on average, while controls gave their jobs an average rating of 6.3.[4] In the interview, these ratings were supplemented with four additional 0–10 scales measuring employed sample members' ratings of various aspects of the job. None of these shows large experimental/control differences.

From a somewhat different perspective, Figures 7.2, 7.3, and 7.4 summarize key characteristics of all 1,836 periods of employment reported by New Chance sample members (both experimentals and controls[5]). Figure 7.2 displays the length of job duration, Figure 7.3 the

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[2] As was briefly discussed in Chapter 2, a problem with the analysis of *duration outcomes* like the time until entry into employment or the duration of employment is that these outcomes were truncated. That is, they could be observed only for the 68.2 percent of all sample members who worked at all during the follow-up period, and they could not be observed after the follow-up period ended. To address this problem, impacts on these outcomes were estimated using a *Tobit* procedure. See Maddala (1983) and Tobin (1958) for details on this procedure.

[3] This table excludes sample members who were never employed. Consequently, as has been indicated, experimental/control comparisons were not fully experimental, and tests of the statistical significance of experimental/control differences were not performed.

[4] As expected, these ratings were higher in cases where this question concerned the current job (a job the sample member still held). For such jobs, these ratings were 6.8 for experimentals and 7.0 for controls.

[5] Analysis of the data underlying these figures showed very little experimental/control difference in employment characteristics.

Table 7.2

Selected Characteristics of the Last or Current Job for New Chance Sample Members Employed at Any Time Within 42 Months After Random Assignment

Measure	Experimentals	Controls
Fringe benefits received (%)		
Sick days with full pay	23.9	24.2
Paid vacations	27.9	30.3
Health plan/medical insurance	21.4	22.5
Dental benefits	17.5	18.8
Transportation to work (%)		
Drive own car	35.7	34.6
Walk to work	10.6	12.1
Get a ride	18.6	20.4
Public transportation	27.4	25.6
Work at home	6.0	5.2
Satisfaction ratings (0-10)		
Overall satisfaction	6.2	6.3
Good opportunities	4.2	4.5
Taught new things	5.4	5.8
Useful skills for next job	5.5	5.7
Good job security	6.0	6.4
Sample size	884	413

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 1,297 sample members for whom there were 42 months of follow-up survey data and who had been employed during follow-up and answered questions about job characteristics. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

Figure 7.2

Distribution of Job Duration in Weeks for New Chance Sample Members Within 42 Months After Random Assignment

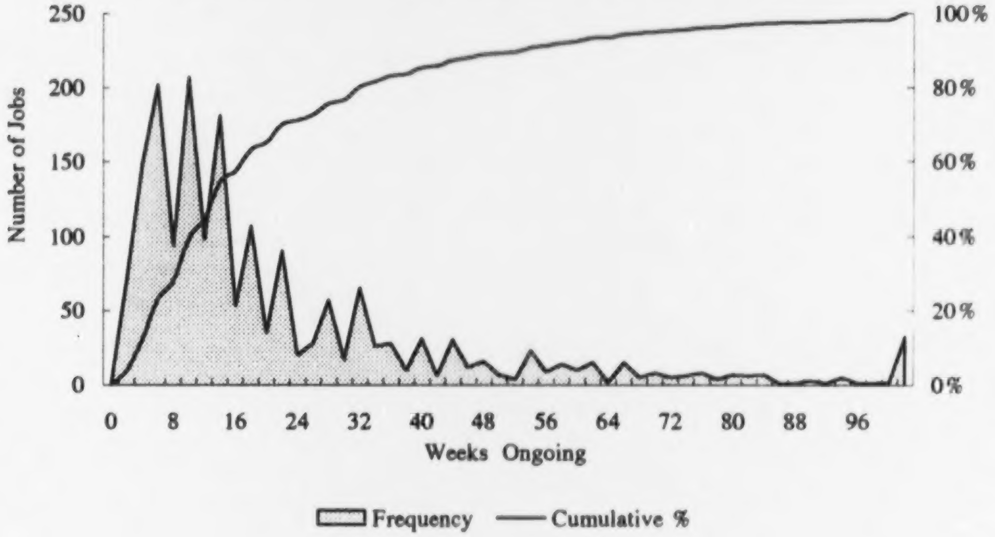
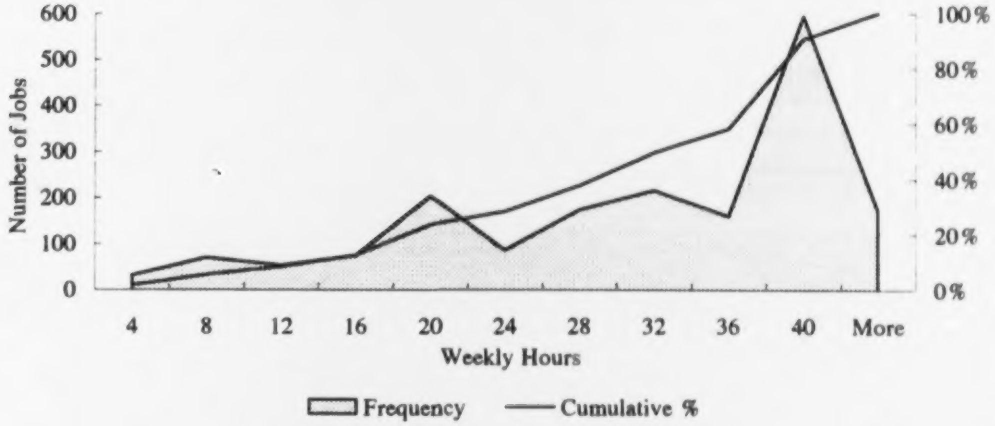


Figure 7.3

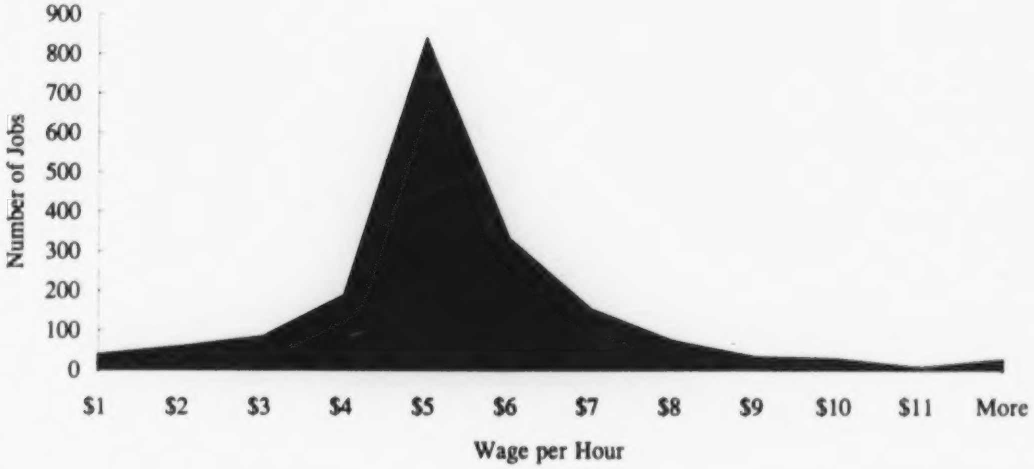
Distribution of Average Hours Worked per Week for New Chance Sample Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Tables G.8 and G.9 for data corresponding to figures.

Figure 7.4

Distribution of Average Hourly Wage for New Chance Sample Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G. 10 for data corresponding to figure.

average number of weekly hours worked in each job, and Figure 7.4 the average hourly wage. Each of these figures is a *histogram,* showing the distribution of a particular outcome across all reported jobs, regardless of whether they represent a sample member's first job, last job, or only job.

Figure 7.2 demonstrates how short the average job held by a New Chance sample member was; over 60 percent of all periods of employment that began after random assignment ended within eighteen weeks, and few employment periods lasted longer than thirty weeks. On the other hand, Figure 7.3 suggests that a substantial share of all jobs held by New Chance sample members were full time; that is, they averaged 30 hours a week or more. Peaks in the figure indicate that jobs with 20, 32, and 40 hours were most common, with about a third of all jobs accounting for 40 weekly hours worked or more.

Finally, Figure 7.4 shows the distribution of the average hourly wage associated with the jobs reported by New Chance sample members. It appears that some jobs paid less than the federal minimum wage, which was $4.25 per hour, and few jobs paid substantially more. Well over half of all jobs paid between $4 and $6 per hour. Thus, in summary, New Chance sample members who were employed held jobs that were often full time but paid little and usually did not last very long.

C. Job Search Behavior for Those Not Employed at Follow-up

Table 7.3 explores the job search behavior of sample members who were not employed at the 42-month follow-up. In the 42-month interview, unemployed sample members who were not looking for work (and therefore technically out of the labor force) were asked to give reasons for their decision not to look for work. The first panel of Table 7.3 presents these different reasons. The reason cited most often for being out of the labor force at follow-up was participation in education or training (35.6 percent of experimentals mentioned this, and so did 36.0 percent of controls[6]). The next three most common reasons were all related to respondents' family responsibilities. Of experi-mentals, more than 25 percent said that child care difficulties (availability or expense) kept them from looking for work. The comparable figure for the control group was 20.7 percent. Similar proportions of experimental and control group members (21.2 and 22.9 percent, respectively) mentioned the fact that they were pregnant or had just had a baby as the reason for their nonparticipation in the labor force. Finally, 18.6 percent of experimentals and 17.4 percent of controls mentioned wanting to stay home with their children as a reason not to look for work.

Sample members who were not employed and not looking for work were also asked when they thought they would be ready to get a job. The table shows that 30.6 percent of experimentals and 26.4 percent of controls estimated being ready for a job within the next six months, while a third of both groups thought they would be ready in the next year. Thirty-five percent of

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[6] Interestingly, only 15.9 percent of experimentals and 16.8 percent of controls actually reported participating in education or training at follow-up.

Table 7.3

New Chance Sample Members' Reasons for Not Working or Looking for Work (For Those Neither Employed Nor Looking for Work at 42 Months After Random Assignment)

Measure	Experimentals	Controls
Reasons for not working and not looking for work (%)		
Pregnant or just had baby	21.2	22.9
Wants to stay home with child	18.6	17.4
Child care too expensive/unavailable	25.8	20.7
School/training	35.6	36.0
Unprepared, need training	12.0	16.4
Health (own/child's)	7.9	2.9
Welfare	1.3	1.5
Other	12.6	12.4
When ready for work (%)		
Next six months	30.6	26.4
Next year	33.3	32.7
Next two years	17.4	22.3
More than two years	17.3	16.7
Never	1.4	1.9
Sample size	609	275

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 884 sample members for whom there were 42 months of follow-up survey data and who were not employed at follow-up and not looking for work. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

Distributions may not total 100.0 percent because of rounding.

experimentals and 39 percent of controls expected to need more than a year to get ready for employment.

In Table 7.4 the focus shifts to those who were not employed but did engage in job search activities. The table gives a breakdown of these activities for experimentals and controls and shows how many hours unemployed sample members who were looking for work spent on this effort in the four weeks prior to the interview: an average of 15.5 for experimental group members and 12.6 hours for their counterparts in the control group. In both groups these job seekers spent most of their time looking at want ads and applying to employers directly.

The bottom panel of Table 7.4 describes the experiences of sample members who were offered a job in the past two years but refused the offer. Of experimentals who were not employed at follow-up, 19.9 percent refused a job offer. The comparable figure for controls was 20.3 percent. The average hourly wage rate for such a job was $5.43 for experimentals and $5.48 for controls. Most often, child care problems were mentioned as the primary reason for the refusal (44.8 percent of job refusals by experimentals and 40.6 percent of such refusals by controls).

D. Job Loss and Reasons for Job Loss

Table 7.5 describes the experiences of sample members who were employed during the follow-up period but were not employed *at* follow-up. First, it shows that most sample members who lost their last job reported that they left on their own (65.3 percent for experimentals and 62.8 percent for controls). Of these sample members, many did so because of problems with child care (19.4 percent of experimentals, 16.3 percent of controls), while interpersonal problems and pregnancy were other important reasons.

E. Impacts on Weeks and Hours Worked

Table 7.6 shows the number of weeks and hours worked by experimentals and controls in the first six months after random assignment and in each of three subsequent years. As expected, given the employment impacts just discussed, New Chance reduced the total number of weeks worked during the first six months. While controls worked an average of 2.55 weeks during this period, experimentals worked only 1.79 weeks, a statistically significant reduction of 0.76 weeks, or almost 30 percent. After the first six months, this significant negative impact on weeks worked disappeared, although experimentals did not work more weeks than controls in any of the three years following, despite a steady increase in work effort over time.

The second panel of Table 7.6 shows impacts on the average number of hours worked in the first six months and the three following years. It appears that the initial significant negative program effect on hours worked persisted for a longer period of time than the reduction in weeks worked. During the first year and a half of follow-up New Chance had a statistically significant negative effect on hours worked amounting to a 29.9 percent difference between experimentals and controls in the first six months after random assignment and a 14.7 percent difference during Months 7 through 18. These figures imply that when experimentals began working after their

Table 7.4

New Chance Sample Members' Job Search Activities and Decisions to Take or Decline Job Offers (For Those Not Employed But Looking for Work at 42 Months After Random Assignment)

Measure	Experimentals	Controls
Job search activities in four weeks prior to 42-month interview (%)		
Looked at want ads	47.1	51.1
Answered/placed want ad	13.7	16.3
Applied to employers directly	65.1	63.7
Asked friends/relatives	18.8	16.8
Checked with agency/school	22.7	20.5
Did nothing	7.1	5.3
Average hours spent looking for work in four weeks prior to 42-month interview	15.5	12.6
Turned down a job in past two years (%)	19.9	20.3
Average hourly wage for that job ($)	5.43	5.48
Reasons for turning down job (%)		
No child care	44.8	40.6
Job did not pay enough	12.4	11.5
No health/fringe benefits	1.0	2.1
Transportation problems	13.4	15.6
Schedule problems	7.0	9.4
Other[a]	39.4	38.6
Sample size	393	190

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 583 sample members for whom there were 42 months of follow-up survey data and who were not employed at follow-up but were looking for work and answered questions about job search activities. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

Distributions may not total 100.0 percent because of rounding.

[a] These included problems with the work schedule, type of work, and other responses.

Table 7.5

Circumstances of New Chance Sample Members' Last Job Loss (For Those Who Had Been Employed But Were Not Employed at 42 Months After Random Assignment)

Measure	Experimentals	Controls
Type of job loss (%)		
Resignation	65.3	62.8
End of job	16.4	21.9
Layoff	12.2	8.7
Dismissed	6.1	6.6
Main reason for leaving last job (among those who resigned) (%)		
Child care	19.4	16.3
Interpersonal problems	14.8	10.6
Pregnancy	12.6	18.7
Did not like job	10.0	9.8
Transportation/inconvenience	7.7	5.7
Sample member moved	6.5	8.1
Low salary	5.8	4.9
School attendance/study	5.8	6.5
Medical reasons	4.8	8.9
Other	12.6	10.6
Sample size	475	206

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 681 sample members for whom there were 42 months of follow-up survey data and who had been employed but were not employed at follow-up and answered questions about job loss. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Tests of the statistical significance of differences in outcomes between experimentals and controls were not performed because these comparisons were not experimental (i.e., the underlying characteristics of the experimentals and controls may have been different).

Distributions may not total 100.0 percent because of rounding.

Table 7.6

Impacts of New Chance on Weeks and Hours Worked Within 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Average number of weeks worked				
Months 1-6	1.79	2.55	-0.76 ***	0.003
Months 7-18	7.17	7.97	-0.80	0.187
Months 19-30	9.94	10.14	-0.19	0.801
Months 31-42	14.26	15.13	-0.87	0.308
Months 1-42	33.20	35.83	-2.64	0.151
Average number of hours worked				
Months 1-6	53.2	75.9	-22.7 ***	0.006
Months 7-18	217.6	255.1	-37.4 *	0.071
Months 19-30	342.0	358.6	-16.6	0.567
Months 31-42	503.8	534.6	-30.8	0.360
Months 1-42	1,117.7	1,225.6	-107.8	0.114
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

participation in New Chance ended, they initially worked fewer hours than employed controls. Again, these significant negative effects disappeared over time, but in neither of the two subsequent years did experimentals work more hours than their control counterparts. Overall, experimentals worked on average 107.8 fewer hours than controls during the 42-month follow-up period as a whole, an 8.8 percent impact that was almost statistically significant (p = 0.114).

F. Impacts on Earnings and Wages

Impacts on sample members' earnings are shown in Figure 7.5 and Table 7.7. The figure clearly illustrates how participation in New Chance depressed the earnings of experimentals during the first half of the follow-up period. While the table shows that the initial negative impacts on earnings were no longer statistically significant after the first year and a half of follow-up, the figure suggests that it took the average experimental nearly two years to reach a point where her monthly earnings were entirely comparable to those of her control group counterparts. It was hypothesized that after this point (the end of the "catch-up" phase) experimentals would begin to earn more than controls as a result of their greater accumulation of human capital from education and training activities, but this hypothesis is not supported by the impacts presented in Figure 7.5 and Table 7.7. Instead, the earnings of experimentals and controls were similar in each of the last two years. Thus, it appears that sample members' initial investment in the program was not followed by a payoff in terms of increased earnings in the long run, at least not for the average experimental.

So far, it seems that the program's lack of impacts on earnings is explained by a corresponding lack of impacts on work effort—that is, on rates of employment and weeks and hours worked. New Chance also may have affected the wage associated with this work effort. The bottom panel of Table 7.7 shows experimental impacts on this outcome for sample members who were employed in each of the last three years of follow-up.[7] While average wage rates increased over time, they did so only modestly. New Chance did not have a statistically significant effect on wage rates in each of the three years for which impacts were calculated.

G. Subgroup and Site Impacts on Earnings

Table 7.8 shows the effects of New Chance on total earnings during the 42-month follow-up period for selected subgroups. The table shows that among the subgroups included in the table, there are none for which the New Chance program had a statistically significant program effect on total earnings, although impact estimates were negative for most subgroups.

There was a great deal of variation in impact estimates across the sixteen sites (not shown in the table), but differences between the impacts across the sites were not statistically significant

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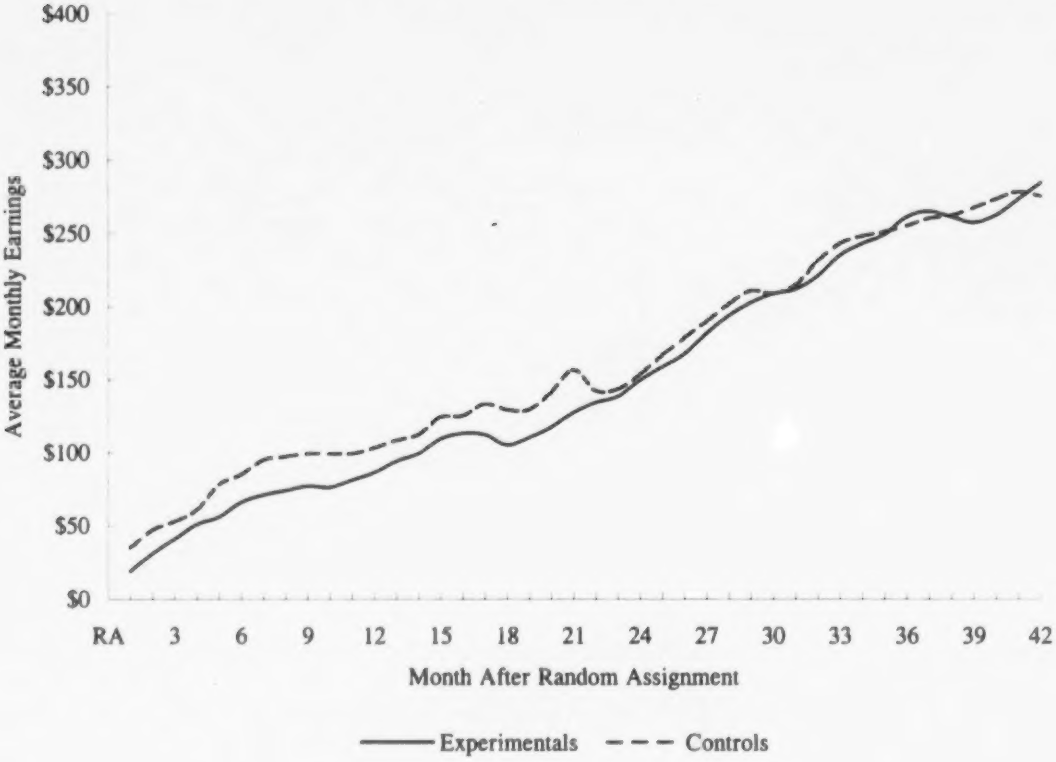
[7] These impacts were estimated using a regression procedure that incorporates a correction for inherent differences between sample members who were employed and sample members who were not. These differences are a potential source of selection bias, which may affect the estimates of experimental impacts on wage rates. The correction used here was first proposed by Heckman (1979).

Figure 7.5

Average Monthly Earnings for New Chance Sample Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.7 for data corresponding to figure.

Table 7.7

Impacts of New Chance on Earnings and Wage Rates Within 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Average total earnings ($)				
Months 1-6	263	358	-95 **	0.033
Months 7-18	1096	1323	-227 *	0.057
Months 19-30	1884	2014	-130	0.462
Months 31-42	3012	3045	-33	0.883
Months 1-42	6255	6741	-486	0.260
Average hourly wage[b] ($)				
Months 7-18	4.86	4.89	-0.03	0.868
Months 19-30	5.41	5.27	0.14	0.640
Months 31-42	5.66	5.68	-0.02	0.929
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] These impacts were estimated using a regression procedure that incorporates a correction for inherent differences between sample members who were employed and sample members who were not. These differences are a potential source of selection bias, which may affect the estimates of experimental impacts on wage rates. The correction used here was first proposed by Heckman (1979).

Table 7.8

Impacts of New Chance on Total Earnings Within 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Total Earnings ($)		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.782
16-17	402	6,262	6,315	-53	0.958		
18-19	997	6,290	7,082	-792	0.201		
20-22	678	6,195	6,496	-301	0.691		
Ethnicity						---	0.520
Black, non-Hispanic	1,087	5,541	5,870	-329	0.579		
Hispanic	474	7,317	7,301	16	0.986		
White or other	515	6,781	8,115	-1334	0.127		
Highest grade completed						-572	0.537
10th or below	1,391	5,603	6,279	-677	0.201		
11th or above	684	7,597	7,702	-104	0.891		
Interval since last attended regular high school						492	0.579
More than 2 years	1,093	6,277	6,605	-328	0.583		
2 years or less	927	6,106	6,926	-820	0.210		
TABE reading test score (grade equivalent)[c]						---	0.990
Below 6th grade	433	4,614	5,028	-414	0.666		
6th or 7th grade	492	5,656	6,111	-455	0.615		
8th or 9th grade	566	6,902	7,611	-710	0.383		
10th grade or above	583	7,355	7,687	-332	0.683		
Family received AFDC when sample member was growing up						---	0.985
Always	341	5,743	6,461	-718	0.510		
Sometimes	970	5,974	6,476	-502	0.418		
Never	749	6,736	7,271	-535	0.459		
Ever employed						899	0.402
Yes	1,646	6,613	7,264	-651	0.183		
No	433	4,930	4,682	248	0.795		
CES-D (depression) Scale[d]						---	0.581
0-15 (not at risk)	967	5,912	6,836	-925	0.150		
16-23 (at some risk)	525	6,566	7,055	-490	0.566		
24-60 (at high risk)	582	6,560	6,410	150	0.853		
Multiple risk score[e]						---	0.670
Low	871	6,317	7,314	-997	0.134		
Moderate	618	5,548	6,068	-520	0.504		
High	525	6,592	6,621	-29	0.973		

(continued)

Table 7.8 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. Although it is possible to assess the statistical significance of variation across multiple subgroups, these differences were not statistically significant.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

as a group (p = 0.182). There were two site-specific impact estimates that were themselves statistically significant. In Harlem, the estimated program effect was a loss in total 42-month earnings of $4,351; in Allentown, the program depressed 42-month earnings by $3,091. Substantial positive impacts on 42-month earnings were found in Chicago ($2,628) and Jacksonville ($2,048), but neither of these estimates was statistically significant. Given the general lack of statistical significance, no attempt was made to further analyze site and subgroup differences.

H. Possible Reasons for the Lack of Impacts on Employment Outcomes

From this discussion of experimental program impacts it appears that New Chance did not meet its goal of improving participants' employment prospects, at least not for the sample as a whole and not within the 42-month follow-up period. Subsequent sections of this chapter include results from nonexperimental analyses that were conducted to identify possible reasons for the lack of employment impacts found so far. These analyses focus on four hypotheses for the lack of employment impacts:

1. Some of the services provided through New Chance may have been valuable, but the service differential associated with New Chance may have been too small to produce significant effects on subsequent employment outcomes.

2. Positive effects on employment outcomes for sample members who successfully completed their participation in New Chance may have been offset by negative effects on employment outcomes for those who did not.

3. Potential program effects on employment outcomes may have been limited by environmental constraints in the lives of New Chance participants, such as child care and transportation problems, welfare rules, and personal problems.

4. The 42-month follow-up period may have been too short to capture the full effect of the New Chance treatment. Young women with parental responsibilities and limited work experience might need a period longer than 42 months to establish themselves firmly in the labor market.

1. Levels of Participation, Education Credentials, and Employment Outcomes. The first and second of these hypotheses may be addressed by analyses that focus on the value in the labor market of services per se. If a greater amount and intensity of services were found to have positive effects on employment outcomes, this finding would support the first hypothesis. If, on the other hand, some services were found to be beneficial while others did not have an apparent payoff, the second hypothesis would be supported.

An instrumental variables estimator[8] was used to estimate the effect of different levels of service receipt on earnings in the last year of follow-up. After correction for possible selection bias, no statistically significant and consistent relationship appeared between these levels of service receipt and earnings during the final year of follow-up. Therefore, within the limitations of this analysis it does not appear that merely increasing the intensity of New Chance services would have improved employment impacts dramatically.[9]

From the instrumental variables analyses it does not appear that more participation in New Chance—that is, a greater service differential—would have produced more favorable program impacts on employment outcomes. Of the experimentals who participated in New Chance education services, however, 30.7 percent received only education (not skills training) and did not complete their participation with an education credential. Therefore, one could hypothesize that while experimentals received more education and training than controls, for many their program experience was too limited to enable them to benefit fully from these services. It is possible to test this hypothesis by measuring the relative employment effects of different types of education and training services, as well as the effects of different education credentials. If only certain types of education and training and certain types of education credentials have a payoff in the labor market, then sample members who spent a substantial time in other types of classes or who ended their participation without a credential might have experienced the opportunity cost of participation without the subsequent payoff. This process, in turn, would have attenuated the net impacts from the New Chance program.

An alternative to the comparison of outcomes across individuals (as presented in the previous section) is the analysis of variation in outcomes over time. By relating changes in employment outcomes to changes in education outcomes, it may be possible to identify a causal relationship between them. The fact that different educational "events" (for example, participation, completion, graduation, and credential receipt) occur at different points in time allows the associated employment effects to be differentiated from each other.[10] The remainder of this section presents the results of such a longitudinal analysis of the relationship between education and employment outcomes.[11]

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[8] Such an estimator replaces the education variable with a predicted ("instrumented") version of this variable, produced with a first-stage regression equation that includes the experimental status variable as one of its independent variables. It is used in Chapters 5 and 8 of this report and discussed in greater detail in Chapter 2.

[9] Note that the statistical power of the instrumental variables method used here is limited. Consequently, the estimated effects of different service levels would have to be very large and consistent to be detected. Also, this comparison of earnings by service level did not differentiate between different types of services received. Finally, the instrumental variables technique is based on the assumption that the entire experimental effect on the outcome variable is channeled through the education variable that is being instrumented. This assumption may not hold in the evaluation of a multifaceted program like New Chance, which could have affected many different aspects of sample members' lives.

[10] Such an analysis can be enhanced by including person-specific "fixed effects" in the regression analysis, a method that explicitly controls for inherent differences in employment outcomes across individuals. A fixed effect is a parameter that is included in analyses to capture nonchanging (fixed) attributes of the individual sample members, thus restricting the source of inference about relationships between education variables and employment outcomes to their variation over time.

Table 7.9 shows results from estimating the earnings effects of several different types of education and training services and education credentials on monthly employment rates, hours worked, and earnings. These effects and the variables they represent are broken down into three different categories: (1) effects of current participation, (2) effects of accumulation of adult education (ABE/GED), skills training, and college, and (3) credential effects associated with a high school diploma, GED, or training certificate.[12] As the top panel of Table 7.9 shows, the effects of current participation in ABE/GED on employment outcomes were negligible, and the effects of participation in training and college were small, albeit statistically significant. For each week of participation in training during a month, the average participant lost only $3.24 in forgone monthly earnings. The comparable figure for participation in college was a loss of $8.10. These numbers suggest that for most sample members participation in ABE/GED, training, or college did not interrupt a period of employment or prevent the participant from starting a job. In other words, the opportunity costs from participation in education or training appear to have been small, at least for the average sample member.

The second panel of the table shows that after controlling for the education credentials that were supposed to accompany such participation, the effects of cumulative education and training were small as well. While the estimated effects of the accumulation of skills training and college were positive, the estimated effect of additional ABE/GED on earnings was *negative*. In other words, within this sample and for this follow-up period, sample members were *less* likely to become employed the more ABE/GED they received.[13] Subsequent participation in skills training and college, however, as well as the receipt of education credentials, produced positive effects on earnings and other employment outcomes.[14] As the third panel shows, the effects of a high school diploma and hours worked were not statistically significant, perhaps because few high school diplomas were received during the follow-up period (most sample members who were high school graduates at follow-up had graduated prior to their application for New Chance).[15] By far the largest employment effect was found for receipt of a trade license or

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[11] See Appendix C for a detailed discussion of the procedure used to generate these estimates.

[12] The latter category also includes trade licenses. College degrees were not included in the analysis, because too few sample members obtained college degrees during the follow-up period. College credits were not included because the survey did not measure *when* they were earned, thus precluding analyses that study their influence over time.

[13] This is a surprising finding that is not consistent with human capital development theory. It is possible that accumulation of adult education (ABE/GED) made sample members less willing to take unskilled jobs while at the same time not providing them with sufficient skills to secure better jobs.

[14] The effect of accumulated weeks of college was larger than the effect of accumulation of skills training, which may be because college credentials (mostly just college credits) were not included in the analysis, whereas training credentials were. Hence, the variable measuring accumulated weeks in college combined the potential positive effects of both college attendance and the receipt of credits and college credentials.

[15] Also, closer study of the experiences of high school graduates showed that positive employment outcomes were concentrated mostly among those who went on to college or skills training. Consequently, the coefficients on the college and skills training variables would capture most of their employment and earnings gains over time.

Table 7.9

Estimated Effects of New Chance Sample Members' Education and Education Credentials on Monthly Employment Outcomes (Controlling for Individual Fixed Effects)

	Regression Coefficient for Monthly Outcome (Standard Error)		
Estimated Effect of:	Employed (%)	Hours Worked	Earnings ($)
Spending a week in adult education (ABE/GED) in the current month	-0.25 **	-0.16	-0.62
	(0.10)	(0.13)	(0.69)
Spending a week in training in the current month	-0.26 *	-0.40 **	-3.24 ***
	(0.14)	(0.18)	(0.98)
Spending a week in college in the current month	-1.00 ***	-1.19 ***	-8.10 ***
	(0.18)	(0.23)	(1.31)
Having spent a week in adult education (ABE/GED) prior to the current month	-0.06 **	-0.15 ***	-0.76 ***
	(0.02)	(0.03)	(0.14)
Having spent a week in training prior to the current month	0.05 *	0.09 ***	0.86 ***
	(0.03)	(0.03)	(0.19)
Having spent a week in college prior to the current month	0.05 **	0.00	0.80 ***
	(0.02)	(0.03)	(0.17)
Having received a high school diploma	4.24 *	4.14	12.18
	(2.25)	(2.87)	(16.05)
Having received a GED	2.57 ***	3.26 ***	22.60 ***
	(0.66)	(0.82)	(4.58)
Having received a training credential (trade license or training certificate)	14.05 ***	17.74 ***	120.91 ***
	(0.89)	(1.13)	(6.31)

SOURCE: MDRC calculations from New Chance survey data.

NOTES: This table shows the estimated effects of unit increases in the education outcomes shown on the left-hand side of the table on the employment outcomes shown at the top of the table. These estimates are regression coefficients obtained from a model relating variation over time in employment outcomes to variation over time in education outcomes. This model controlled for variation in individual characteristics by including person-specific fixed effects. The regression was estimated with one autoregressive parameter, using PROC AUTOREG in SAS.

Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

certificate. Such a credential was estimated to increase monthly earnings by $120.91, a very substantial effect considering that average monthly earnings at the end of the follow-up period were $283 for experimentals and $274 for controls.

Table 7.10 translates these numbers into several simulated earnings profiles for a hypothetical sample member engaged in different combinations of education and training. Each of the five columns in this table presents a different scenario. In the first four scenarios, the hypothetical sample member is assumed to have participated in ABE/GED for 26 weeks, followed by attainment of a GED in the first three scenarios. In addition to this ABE/GED package, the first column adds the effect of 26 weeks of training completed with a trade license, and the second column includes the simulated effect of two years of college. The bottom row of this table shows the total estimated net payoff or loss (as captured by individual earnings) for the 42-month follow-up period.[16] It appears that, with a net effect of $4,314, the skills training scenario has the greatest payoff, and most of this is explained by the effect of the training credential. The estimated effect of a combination of adult education (ABE/GED) and two years of college, presented in the second column, is a net payoff of $1,139. In contrast, the third and fourth columns, which present scenarios in which participation in education does not progress beyond attainment of the GED, show small or even negative net effects on total earnings during the follow-up period. The final column shows how this simulation plays out for a "typical" New Chance sample member (receiving 25 weeks of basic education, 12 weeks of skills training, 9 weeks of college, and a GED certificate).

The results from this nonexperimental time-series analysis offer important potential explanations for the lack of experimental program impacts on employment outcomes, congruent with the second hypothesis formulated earlier in this section. As was described in Chapters 3 and 4, the program experience of many New Chance participants did not progress beyond participation in ABE/GED and receipt of a GED. The present analyses suggest that these outcomes produce positive effects on employment outcomes only if they are used to gain subsequent access to services that have greater value in the labor market.[17]

2. Other Determinants of Variation in Employment Outcomes. New Chance attempted to increase sample members' earnings and improve their employment outcomes by providing remedial education (ABE/GED) and skills training. As the third hypothesis suggests, however, the human capital attributes targeted by these services are not the only determinants of labor market success. In fact, they may not even be the most important. The following sections of this chapter explore relationships between employment outcomes, on the one hand, and fertility status and living arrangements, on the other. As noted in Chapter 6, these outcomes are important because they had the potential to expand sample members' opportunities in the labor market or to limit them and thus affect the payoff from the human capital development efforts just analyzed.

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[16] Note that the simulated earnings effects presented in Table 7.10 are sensitive both to variation in the estimates underlying them and to assumptions about the scenarios themselves. Therefore, these figures should be interpreted with caution.

[17] This finding is consistent with Murnane and Willett, 1993, and Murnane, Willett, and Boudett, 1994.

Table 7.10

Simulated Cumulative Effects of New Chance Sample Members' Education and Education Credentials on 42-Month Earnings

	Simulated Cumulative Effect on 42-Month Earnings of Different Hypothetical Combinations of Education and Education Credentials				
Simulated earnings effect of:	**Scenario I:** 26 Weeks of Adult Education, a GED, 26 Weeks of Training, and a Training Credential ($)	**Scenario II:** 26 Weeks of Adult Education, a GED, and 2 Years of College ($)	**Scenario III:** 26 Weeks of Adult Education and a GED ($)	**Scenario IV:** 26 Weeks of Adult Education, but no GED ($)	Simulated Cumulative Effect of the Average Amount of Education and Training Received by New Chance Sample Members[a] ($)
The specified number of weeks invested in:					
Adult education (ABE/GED)	-16	-16	-16	-16	-16
Training	-84				-39
College		-842			-73
The human capital accumulated in:					
Adult education (ABE/GED)	-771	-771	-771	-771	-744
Training	744				351
College		1,955			225
Having received a GED	814	814	814		814
Having received a training credential (trade license or training certificate)	3,627				
Total net payoff/loss	4,314	1,139	26	-788	519

SOURCE: MDRC calculations from New Chance survey data.

NOTES: The numbers in this table represent hypothetical cumulative effects of education outcomes on earnings, calculated by summing the monthly effects shown in Table 7.9 for a typical period of time during which these effects might be experienced. For example, the payoff to receiving a GED would not begin to accumulate until after the GED has been received—i.e., after 26 weeks of adult education, according to this scenario. The result is a cumulative total that can be compared more easily with outcomes presented elsewhere in this report. Tests of the statistical significance of these estimates were not conducted.

[a] This amounts to 25 weeks of adult education, 12 weeks of training, 9 weeks of college, and a GED.

Specifically, as was discussed earlier, concerns about child care and additional pregnancies and births often were mentioned as reasons why sample members left a job or were not looking for one.[18]

Because of data constraints, the analyses presented in this section are less conclusive than the ones presented so far. Aside from the fact that these analyses are nonexperimental in nature, they are based on imperfect data, because living arrangements and socioemotional outcomes (such as social support and depression) were not measured continuously during the 42-month follow-up period. Consequently, it is difficult to establish a temporal order between these variables and the employment outcomes, which would clarify the direction of causal links if they existed.

3. **Living Arrangements and Earnings at Follow-up.** Figure 7.6 explores the relationship between sample members' living arrangement at the time of the 42-month follow-up interview and their earnings during the last six months of follow-up (Months 37–42). Given the importance of living arrangements as a source of practical and emotional support, one would expect to see substantial variation in employment outcomes across different living arrangements. A sample member who lives alone with her children typically bears more responsibility for child care arrangements, for household chores, and for maintaining financial stability than do sample members who live with their parents or who live with a husband or a partner. These responsibilities may become barriers to employment, resulting in lower earnings.

On the other hand, an inverse causal relationship is also possible. A sample member who holds a job and has higher individual earnings would also be better able financially to move out of her parents' household and set up her own, either with or without a spouse or unmarried partner. It is not possible to choose between these alternative explanations with the data collected at the 42-month interview, but Figure 7.6, which shows earnings during Months 37–42 broken down by living arrangement at follow-up, points toward the first hypothesis, suggesting an enabling effect of certain living arrangements on participation in the labor force, rather than the other way around.[19] It appears that the earnings for sample members who lived alone with their children were significantly lower than those for sample members who lived with their mothers or with other adults. Sample members who were married at the time of the 42-month follow-up interview had the highest earnings during the six months preceding the end of the follow-up

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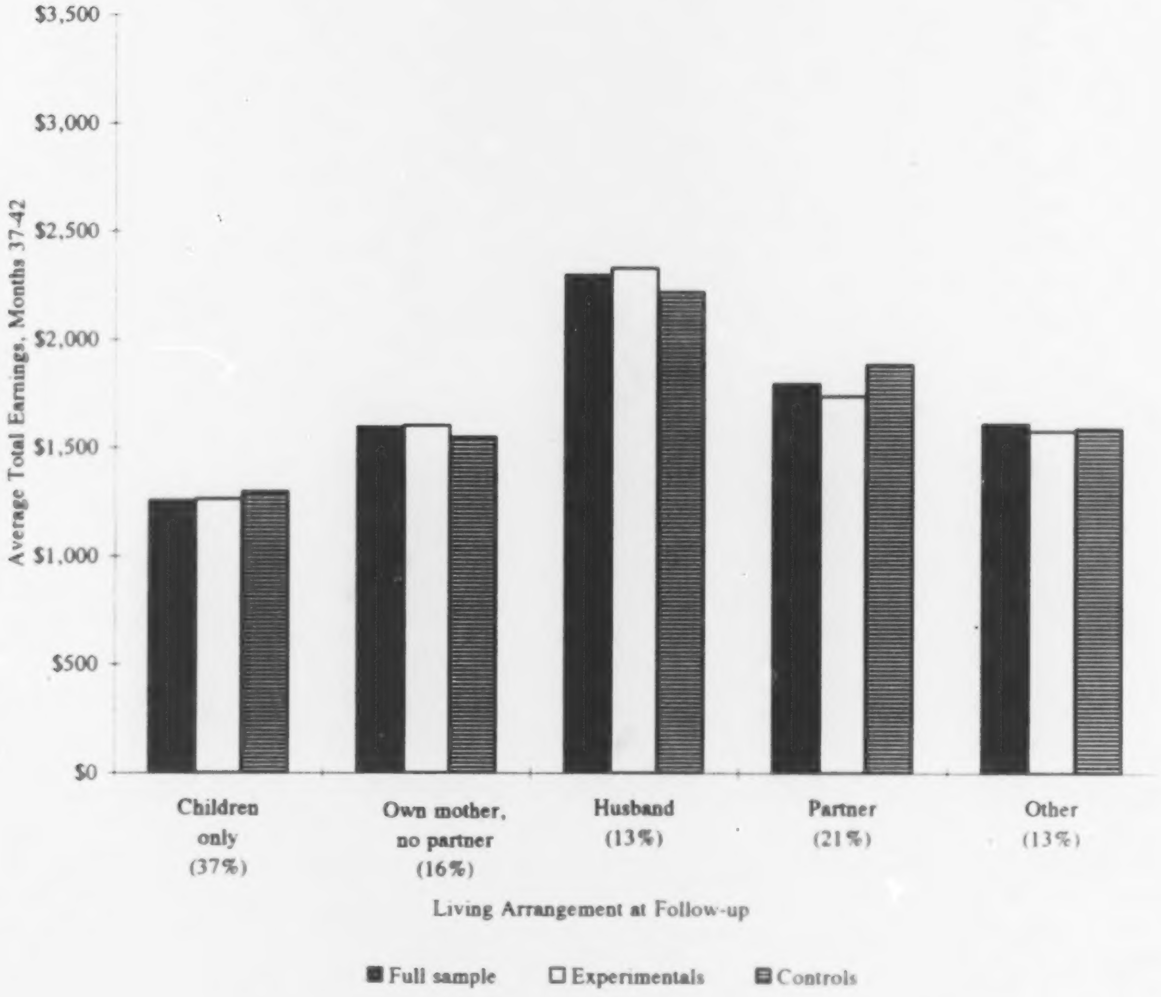
[18] Remember that more than 40 percent of all job refusals were child care–related, as were more than 18 percent of job losses. Of all sample members who were not working and not looking for work, more than 24 percent mentioned child care concerns as the main reason not to pursue employment.

[19] Figures 7.6 and 7.7 display regression-adjusted earnings estimates obtained from an ANOVA (Analysis of Variance) procedure, using the usual 51 baseline characteristics as control variables. Unfortunately, these controls may not eliminate the possibility of selection bias from unmeasured characteristics. Sample members who were married at follow-up may have been more likely to do well in the labor market even before they got married. To assess the possible extent of this bias, the analysis was repeated with earnings during Months 1 through 18 as a "control" variable. During these early months 62.8 percent of those married were not yet married and 46.2 percent of those living alone with their children lived in households with adults. This adjustment did not change the estimates presented in Figure 7.6 in any fundamental way, suggesting that most of the differences shown are indeed directly associated with sample members' living arrangements at follow-up.

Figure 7.6

Average Earnings of New Chance Sample Members Within Months 37-42 After Random Assignment, by Living Arrangement at 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.11 for data corresponding to figure.

period. With an average of $2,293, their earnings were almost twice as high as those of sample members who lived alone with their children ($1,255).

4. **Fertility Outcomes and Earnings.** Sample members' fertility outcomes during the follow-up period constituted another important potential predictor of their employment outcomes. The New Chance program actively (although not successfully) attempted to motivate program participants to postpone childbearing until after they gained a foothold in the labor market. It was believed that additional births would interfere with sample members' progress toward self-sufficiency, by affecting their educational development and keeping them out of the labor force. As was shown in Chapter 6, many sample members in both research groups (54.7 percent of experimentals and 55.3 percent of controls) gave birth during the follow-up period, and an even greater number (75.2 and 72.8 percent of experimentals and controls, respectively) became pregnant.

Figure 7.7 shows the relationship between pregnancies and births during the follow-up period and earnings in the last six months of follow-up. The figure explores this relationship as it varies across different living arrangements. This breakdown by living arrangement reflects the assumption that the effects of additional pregnancies and births would vary depending on sample members' living arrangements and social supports.

In four of the five living arrangement categories shown in Figure 7.7, a post-baseline pregnancy per se did not appear to have a statistically significant effect on earnings during the last six months of follow-up. Of those who became pregnant, however, sample members who gave birth during the follow-up period had significantly lower earnings than sample members who did not.[20] Only after resulting in a birth did repeat pregnancies become a significant factor in the employment behavior observed for the young women in the sample.

The bars shown in Figure 7.7 are labeled to indicate the number of sample members who fell in each category. According to these numbers, it appears that the groups that were more disadvantaged in terms of their earnings during the last six months of follow-up were also the largest. If barriers to employment explain the differences shown in Figure 7.7, then a substantial proportion of all sample members were facing such barriers at the end of the follow-up period. Chapter 6 showed that New Chance did not ameliorate this situation. Hence, additional births and living arrangements that were unsupportive of employment may have seriously reduced the program's potential to improve the employment outcomes of the young women it served.

5. **Developments Beyond the 42-Month Follow-up Period.** In the introduction to this section, it was hypothesized that the follow-up period used to evaluate the New Chance program may not have been long enough to capture fully the economic benefits of participation in education and training. If the follow-up period were too short, the employment findings presented so far might have underestimated the actual long-term program effects on employment

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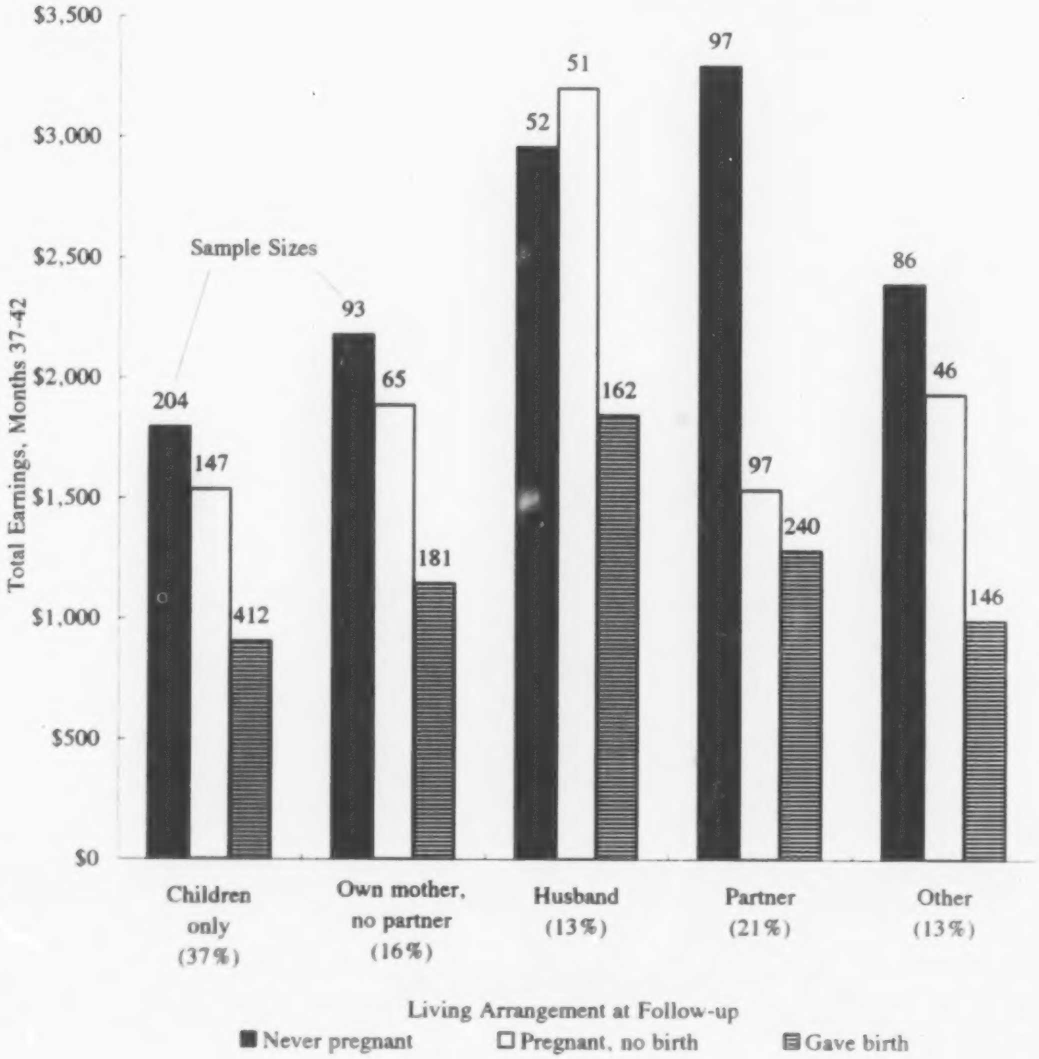
[20] Sample members living with a partner (but not a husband) at follow-up were the exception. If they became pregnant, their earnings were not statistically significantly different whether they then gave birth or not.

Figure 7.7

Average Earnings of New Chance Sample Members Within Months 37-42 After Random Assignment, by Living Arrangement and Fertility Status at or Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.12 for data corresponding to figure.

outcomes. There are three reasons why this may have been the case in this evaluation of New Chance:

1. Many experimental group members continued to participate in education and training even after most ended their participation in New Chance, thus accumulating unrealized economic benefits.

2. The process of gaining a foothold in the labor market and increasing one's wages is very gradual, especially for economically and educationally disadvantaged young mothers.

3. Sample members' attempts to enter employment may have been delayed by other factors, including recent births, child care responsibilities, a preference for staying home with children, or simply a lack of maturity.

The available data may be used to explore the plausibility of these possible causes for a delay in program effects. As far as the first explanation—continued participation—is concerned, Chapter 4 showed that at the time of the 42-month follow-up, experimental and control group members in equal proportions were enrolled in skills training or college. This was also the case in each of the two years preceding the 42-month interview. Therefore, by the end of the follow-up period the unrealized potential benefits from ongoing education and training probably were similar for the two research groups, so this explanation must be ruled out.

The second possible reason to expect program effects beyond the end of the 42-month follow-up period—the fact that earnings gains may take a long time to materialize—can be explored by comparing earnings gains over time for experimental and control group members. The first panel of Table 7.11 does so by comparing across research groups the increase in average monthly earnings over six months. Each line in this panel shows the increase in earnings for a month in the last year of follow-up, compared with the month preceding it by six. Thus, the first line shows the difference between average earnings in Month 31 and average earnings in Month 25, the second the difference between earnings in Months 32 and 26, and so forth. It becomes apparent quickly that there are no clear trends or significant experimental/control differences in these comparisons. In other words, during the last year of follow-up, experimental group members do not appear to have experienced greater earnings *growth* than their control counterparts.

The third possible source of delayed effects mentioned above is the hardest to dismiss or support with the available 42-month data. Some sample members may postpone their entry into the job market in spite of having learned new skills, obtained diplomas, and stopped participating in education and training. This possibility is pursued in the bottom panel of Table 7.11. Shown are the program effects on the joint probability of having a training credential, no work, and no intention to work at follow-up. It appears that more experimentals (6.2 percent) than controls (3.8

Table 7.11

Impacts of New Chance on Changes in Earnings During Months 31-42 After Random Assignment, Compared with Earnings Six Months Earlier, and Impacts on Having Training Credentials or College Credits and Neither Working Nor Looking for Work at 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Change in average monthly earnings compared with six months prior to the month in question ($)				
Month 31	53	48	5	0.767
Month 32	54	53	1	0.950
Month 33	53	52	1	0.982
Month 34	50	46	4	0.848
Month 35	46	39	7	0.697
Month 36	53	47	6	0.756
Month 37	54	45	9	0.630
Month 38	39	30	9	0.628
Month 39	22	25	-3	0.903
Month 40	18	25	-7	0.707
Month 41	24	27	-3	0.872
Month 42	23	20	3	0.872
Has a trade license or training certificate but is neither employed at follow-up nor looking for work (%)	6.2	3.8	2.4 **	0.021
Has college credits but is neither employed at follow-up nor looking for work (%)	5.2	3.9	1.4	0.176
Sample size	1,401	678		

SOURCE: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

percent) were in this situation, a difference that was statistically significant.[21] When reasons for their nonparticipation in the labor force were examined, both experimentals and controls mentioned child care problems and recent births as their main reason for remaining out of the labor market in spite of their training credentials. The program did not have a statistically significant effect on the probability of not working and not looking for work despite having college credits. The bottom line, however, is that (at 11.4 percent) the proportion of experimentals who had advanced credentials and were not in the labor force at follow-up was too small to foreshadow substantial increases in employment and earnings beyond the 42-month follow-up period.

Thus, it seems unlikely that substantial program effects on earnings and employment occurred after the end of the 42-month follow-up period and outside the scope of this report. In summary, experimentals and controls had similar levels of participation in the last year of follow-up, there were no experimental/control differences in the rate of entry into employment, experimentals did not show stronger earnings growth, and at follow-up relatively few experimentals were unemployed despite having a credential that would enhance their ability to get a job.

III. Welfare Outcomes

A. Impacts on Welfare Receipt

Table 7.12 and Figure 7.8 show impacts on welfare receipt during the 42-month follow-up period. While the percentage receiving AFDC during each month declined over time, the majority of New Chance sample members remained on AFDC at the end of the follow-up period. During Month 42, 75.4 percent of experimentals and 73.5 percent of controls received AFDC. In the third "post-program" year (Months 31 through 42), 84.7 percent of experimentals and 85.4 percent of controls received AFDC for at least a month.

Throughout most of the follow-up period, the impact of New Chance on AFDC receipt was small.[22] During the first six months of the follow-up period, 96.2 percent of experimentals reported receiving AFDC, compared with 94.4 percent of the control group, a difference of 1.7 percentage points that was statistically significant. In the year following this predominantly in-program period, 94.9 percent of experimentals and 93.0 percent of controls received AFDC, a difference of 1.9 percentage points, also statistically significant. Figure 7.8 shows that

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[21] New Chance experimental group members were also more likely to have college credits, no work, and no intention to work, but the difference was not statistically significant.

[22] It is important to acknowledge that the New Chance follow-up surveys measured only *whether* sample members were receiving AFDC in a particular follow-up month, not the *amount* of welfare received (except in the month prior to the interview). Therefore, it is possible that the program may have increased or reduced the amount of welfare received by recipients throughout the follow-up period; this fact would not be apparent from the follow-up data that were collected.

Table 7.12

Impacts of New Chance on AFDC Receipt Within 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Ever received AFDC (%)				
Months 1-6	96.2	94.4	1.7 **	0.039
Months 7-18	94.9	93.0	1.9 *	0.066
Months 19-30	91.2	90.5	0.7	0.596
Months 31-42	84.7	85.4	-0.7	0.681
Months 1-42	98.9	97.9	1.0 **	0.044
Average number of months on AFDC				
Months 1-6	5.59	5.40	0.19 ***	0.002
Months 7-18	10.49	10.36	0.13	0.396
Months 19-30	9.92	9.89	0.03	0.884
Months 31-42	9.17	9.10	0.07	0.728
Months 1-42	35.17	34.76	0.41	0.367
Ever combined AFDC with work (%)				
Months 1-6	13.0	16.3	-3.3 **	0.041
Months 7-18	31.8	31.0	0.8	0.698
Months 19-30	31.6	28.9	2.6	0.221
Months 31-42	37.5	34.5	2.9	0.187
Months 1-42	61.8	57.4	4.4 **	0.050
Average number of months combining AFDC and work				
Months 1-6	0.39	0.51	-0.12 **	0.037
Months 7-18	1.24	1.28	-0.04	0.751
Months 19-30	1.37	1.38	-0.01	0.946
Months 31-42	1.75	1.87	-0.12	0.415
Months 1-42	4.76	5.04	-0.29	0.367
Movement off of AFDC (%)				
Ever left AFDC	46.8	47.3	-0.6	0.788
Ever left AFDC for 3 months or more	39.4	39.9	-0.5	0.806
Ever left AFDC for 6 months or more	32.0	32.6	-0.6	0.781
Ever left AFDC for 12 months or more	22.0	22.8	-0.8	0.648
Ever left AFDC for 24 months or more	8.1	9.5	-1.5	0.246
Sample size	1,401	678		

(continued)

Table 7.12 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

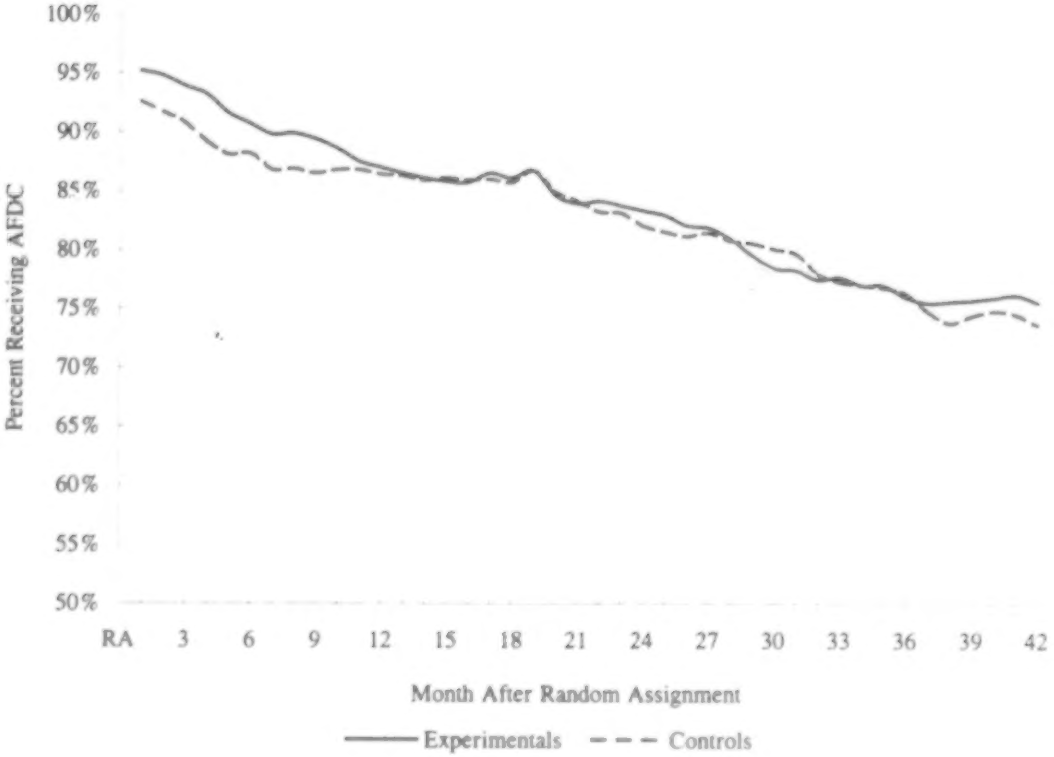
NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Figure 7.8

Monthly Rates of AFDC Receipt for New Chance Sample Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.13 for data corresponding to figure.

differences were concentrated in the first nine months of follow-up. After that, experimentals and controls reported similar rates of AFDC receipt.

A different measure of AFDC receipt is presented in the second panel of Table 7.12, which shows the number of months sample members were receiving AFDC during the first six months of follow-up and in the three years following. On average, both groups received AFDC for more than 80 percent of the follow-up period: 35.2 months for experimentals and 34.8 months for controls. (The small experimental/control difference was not statistically significant.)

Experimentals were more likely than controls to combine work and welfare in at least one of the 42 months of follow-up. Of the experimental group, 61.8 percent did so in any of the forty-two months of follow-up, compared with 57.4 percent of the control group. This difference of 4.4 percentage points was statistically significant. During the first six months of the follow-up period, however, controls were significantly more likely to combine welfare and work, and they did so for more months. Throughout the follow-up period, controls recorded more months of combining work and welfare, although the difference over 42 months was not statistically significant.

The bottom panel of Table 7.12 shows program impacts on several measures of movement off welfare during the 42-month follow-up period. It appears that 46.8 percent of experimentals and 47.3 percent of controls ever left welfare for at least a month during the follow-up period. The difference was not statistically significant. Only 32.0 percent of experimentals remained off welfare for six months or more, a figure similar to that for controls, 32.6 percent.

B. Subgroup and Site Impacts on Welfare Receipt

Table 7.13 presents program impacts on the number of months of AFDC receipt for various subgroups. Generally, these impacts were not statistically significant and only once was the difference in impacts across subgroups statistically significant (for subgroups defined by the interval since last attended high school). New Chance significantly increased the number of months of AFDC receipt (by 1.5 months) for sample members who were out of school for two years or less at baseline.

In a breakdown by site (not shown in the table), New Chance had significant effects only in Jacksonville and Portland. In Jacksonville, the program reduced AFDC receipt by a statistically significant 3.3 months, while it increased AFDC receipt by 3.3 months in Portland. Cross-site differences in estimated impacts were not statistically significant. Since no clear pattern of subgroup and site impacts emerged, no further analyses were conducted to explain the few significant differences found.

Table 7.13

Impacts of New Chance on Number of Months Receiving AFDC Within 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Number of Months Receiving AFDC		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.359
16-17	402	35.6	33.9	1.7	0.106		
18-19	997	34.1	34.1	-0.1	0.930		
20-22	678	36.5	36.3	0.3	0.729		
Ethnicity						---	0.743
Black, non-Hispanic	1,087	37.0	36.8	0.2	0.718		
Hispanic	474	33.9	33.8	0.1	0.899		
White or other	515	32.4	31.4	1.0	0.274		
Highest grade completed						0.2	0.877
10th or below	1,391	35.5	35.1	0.4	0.427		
11th or above	684	34.5	34.2	0.3	0.715		
Interval since last attended regular high school						-2.0 **	0.034
More than 2 years	1,093	34.8	35.3	-0.5	0.445		
2 years or less	927	35.6	34.1	1.5 **	0.029		
TABE reading test score (grade equivalent)[c]						---	0.236
Below 6th grade	433	35.6	35.9	-0.3	0.753		
6th or 7th grade	492	35.5	36.4	-0.9	0.337		
8th or 9th grade	566	35.4	34.1	1.4	0.116		
10th grade or above	583	34.3	33.2	1.1	0.206		
Family received AFDC when sample member was growing up						---	0.684
Always	341	36.0	34.9	1.1	0.334		
Sometimes	970	35.5	35.0	0.5	0.413		
Never	749	34.4	34.5	0.0	0.958		
Ever employed						-0.8	0.498
Yes	1,646	34.9	34.3	0.6	0.284		
No	433	36.4	36.6	-0.2	0.831		
CES-D (depression) Scale[d]						---	0.882
0-15 (not at risk)	967	35.0	34.6	0.4	0.536		
16-23 (at some risk)	525	35.1	35.1	0.1	0.954		
24-60 (at high risk)	582	35.5	34.8	0.7	0.429		
Multiple risk score[e]						---	0.140
Low	871	35.2	33.8	1.4 *	0.051		
Moderate	618	35.9	35.6	0.3	0.728		
High	525	34.4	35.3	-0.9	0.325		

(continued)

Table 7.13 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

C. Possible Reasons for the Lack of Impacts on Welfare Receipt

Presumably, young women's decisions to leave welfare are driven by two factors: (1) their family situations and (2) their employment and earnings. Figure 7.9 shows the proportion of sample members (experimentals and controls combined) receiving AFDC in the month prior to the final interview, broken down by living arrangement and employment status.[23] As expected, sample members who were employed at follow-up reported lower levels of AFDC receipt than sample members who were not employed. This finding was true for each of the five living arrangements shown in Figure 7.9. Among these living arrangements, women who lived alone with their children were most likely to receive AFDC at follow-up (54.2 percent of those employed and 91.6 percent of those not employed). Women who were married were least likely to be on AFDC at follow-up. Almost half of all unemployed married women (45.7 percent) continued receiving AFDC at follow-up, however, while 27.0 percent of those who worked supplemented their earnings with AFDC. Also of interest in Figure 7.9 is the substantial difference in welfare receipt between sample members who were married and sample members who were living with a partner but were not married. Especially for young women who were not working at follow-up, marriage appeared to substantially reduce their dependence on AFDC.

Thus, the lack of experimental impacts on AFDC receipt is consistent with the program's lack of effects on earnings and employment. And as the previous chapter showed, New Chance did not increase the proportion of young women who were living in the kind of supportive living arrangements that might have reduced their dependency on AFDC.

IV. Impacts on Family Income

A. Income Sources

Tables 7.14 and 7.15 contain a detailed breakdown of family income[24] reported by sample members for the month prior to the 18- and 42-month interviews. Table 7.14 shows that the proportion of sample members reporting income from their own earnings grew from about one in six to one in three between the 18- and 42-month interviews. The proportion receiving income from a partner's earnings increased as well; at 18 months, 15.6 percent of experimentals and 13.1 percent of controls reported earnings from a husband or partner. At 42 months, 22.8 percent of experimentals and 26.1 percent of controls had income from their partner's earnings. In the month preceding the 42-month interview, the experimental/control difference in income from employment by a husband or partner was statistically significant.

As income from earnings increased from the first follow-up interview to the second, income from public sources diminished. The number of sample members reporting income from

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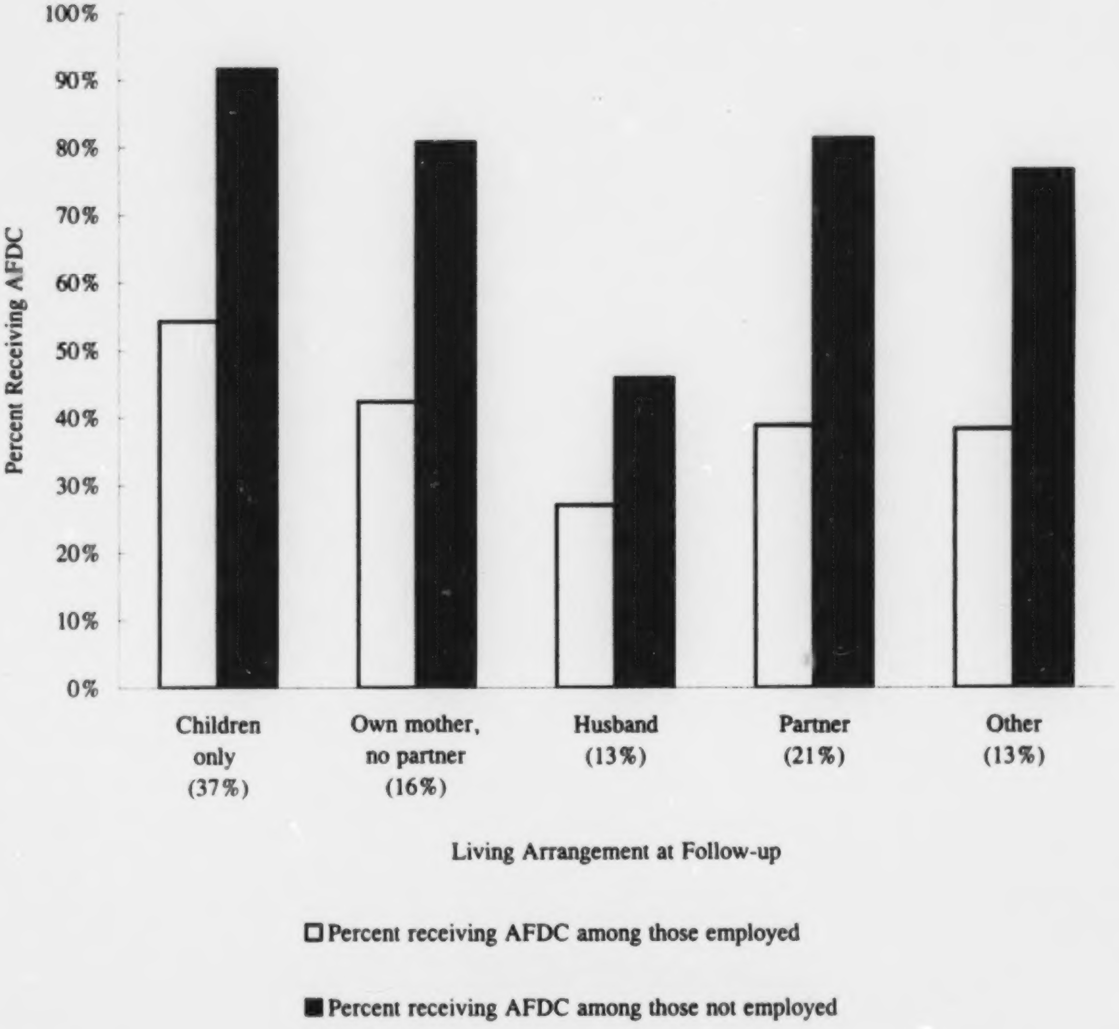
[23] The estimates underlying this graph were obtained from a regression procedure using 51 baseline control variables.

[24] Measures presented here include only the sample member's income, that of her husband or partner if she had one, and that of her children. It did not include income of other members of the household in which she lived.

Figure 7.9

AFDC Receipt by New Chance Sample Members in Month 41 After Random Assignment, by Living Arrangement and Employment Status at 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data.
See Appendix Table G.14 for data corresponding to figure.

Table 7.14

Impacts of New Chance on Availability of Family Income Sources at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Income sources reported as used during the month prior to the 18-month interview (%)				
Sample member's employment	16.1	17.2	-1.1	0.551
Husband or partner's employment	15.6	13.1	2.4	0.143
Sample member's AFDC case	83.1	82.6	0.4	0.811
Husband's or partner's AFDC case	1.0	1.2	-0.2	0.722
Food stamps	86.9	86.2	0.7	0.669
Supplemental Security Income (SSI)	2.8	1.5	1.2 *	0.089
Unemployment or worker's compensation	1.0	1.1	-0.1	0.871
Alimony or child support	11.6	11.5	0.0	0.976
Private or government pension	0.7	0.5	0.2	0.552
Family or friends	11.2	11.0	0.3	0.863
Public housing or rent assistance	35.2	30.5	4.7 **	0.019
Income sources reported as used during the month prior to the 42-month interview (%)				
Sample member's employment	27.8	30.9	-3.1	0.124
Husband or partner's employment	22.8	26.1	-3.3 *	0.092
Sample member's AFDC case	70.2	68.3	2.0	0.325
Husband's or partner's AFDC case	1.7	2.1	-0.3	0.588
Food stamps	79.6	77.8	1.8	0.324
Supplemental Security Income (SSI)	5.3	3.2	2.1 **	0.036
Unemployment or worker's compensation	1.4	1.0	0.4	0.404
Alimony or child support	12.3	12.0	0.3	0.840
Private or government pension	1.4	1.0	0.4	0.404
Family or friends	14.1	16.2	-2.2	0.187
Public housing or rent assistance	38.2	37.5	0.7	0.731
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table 7.15

Impacts of New Chance on Family Income at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Average income from selected sources during the month prior to the 18-month interview ($)				
Sample member's earnings	105	129	-24 *	0.073
Husband or partner's earnings	160	132	28	0.165
Sample member's AFDC grant	327	327	0	0.975
Husband's or partner's AFDC grant	2	3	0	0.842
Food stamps	164	167	-3	0.425
Supplemental Security Income (SSI)	12	7	4	0.178
Unemployment or worker's compensation	4	3	0	0.904
Alimony or child support	11	11	0	0.949
Private or government pension	3	1	2	0.253
Family or friends	18	16	2	0.598
Average income from selected sources during the month prior to the 42-month interview ($)				
Sample member's earnings	283	274	9	0.697
Husband or partner's earnings	295	353	-58 *	0.063
Sample member's AFDC grant	287	280	8	0.439
Husband's or partner's AFDC grant	4	4	0	0.987
Food stamps	177	177	0	0.986
Supplemental Security Income (SSI)	23	16	7	0.105
Unemployment or worker's compensation	7	4	3	0.324
Alimony or child support	14	13	1	0.822
Private or government pension	5	4	1	0.558
Family or friends	18	25	-7 *	0.060
Average total income in the month prior to ($)				
The 18-month interview	804	793	11	0.688
The 42-month interview	1,113	1,150	-36	0.329
Distribution of total monthly income (month 42) (%)				
Less than $600	23.5	22.2	1.3	0.491
$601-$900	29.9	31.1	-1.1	0.599
$901-$1,500	23.2	23.1	0.1	0.979
More than $1,500	23.4	23.6	-0.3	0.893
Sample size	1,401	678		

(continued)

Table 7.15 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a]A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

AFDC and food stamps remained high, however, even at the 42-month follow-up interview: 70.2 percent of experimentals reported receiving AFDC, against 68.3 percent of controls. Food stamp receipt in the month prior to the 42-month interview was reported by 79.6 percent of experimentals and 77.8 percent of controls. Neither one of these differences was statistically significant.

The proportion of sample members reporting income sources other than earnings, AFDC, or food stamps was relatively small at both follow-up interviews. At 18 months, New Chance was found to have a significant effect on the proportion of sample members living in public housing or reporting the receipt of housing assistance. At that point 35.2 percent of experimentals lived in public housing or received housing assistance, as opposed to 30.5 percent of controls.[25] At 42 months, these proportions had increased slightly (to 38.2 and 37.5 percent of experimentals and controls, respectively), and the experimental difference was no longer statistically significant. Finally, there was a small but statistically significant difference in the percentage receiving Supplemental Security Income (SSI) at 42 months. These benefits provided income to 5.3 percent of the experimental group, compared with 3.2 percent of controls.

As Table 7.15 shows, in the two years following the 18-month interview total reported monthly income increased from $804 to $1,113 for experimentals and from $793 to $1,150 for controls. For both groups, these increases were due mainly to increases in average reported earnings. Total reported earnings (from the sample member and her partner, total not shown in the table) grew from $265 to $578 for experimentals and from $262 to $627 for controls. At the 18-month follow-up, experimentals reported earnings of their own that were statistically significantly lower than those reported by members of the control group ($105 versus $129). By the time of the 42-month interview, this difference had disappeared. At 42 months, however, experimentals reported lower earnings from their husbands and partners. While controls received $353 from this source, experimentals received only $295, a substantial and statistically significant difference of 16.5 percent. The bottom panel of Table 7.15 examines the distribution of monthly income at the 18- and 42-month interviews. New Chance did not affect this distribution.

B. Other Measures of Material Well-being

The 42-month follow-up survey contained several other measures of material well-being. These included a material hardship scale[26] and several measures of acute financial need, such as a temporary inability to pay for phone service, health care, or food. Table 7.16 presents program

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[25] Case managers in some sites helped New Chance participants secure public housing or housing assistance.

[26] The material hardship scale is the sum of eight dichotomous variables that measure whether a sample member (1) could not afford to buy food, (2) spent less than the USDA's 1994 thrifty food budget, (3) had no health insurance, (4) could not afford medical attention when needed, (5) could not afford to see a dentist when needed, (6) lived in an overcrowded home, (7) had her gas or electricity turned off, or (8) had housing problems due to cost or disputes with her landlord. For the full sample, the number of hardships ranged from zero to seven, with an average of 1.5 and a standard deviation of 0.98 (see Mayer and Jencks, 1989). Many of the items are also included in Table 7.16 individually.

Table 7.16

Impacts of New Chance on Measures of Hardship at or Within 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Score on hardship scale (out of 8)	1.6	1.5	0.1 *	0.054
Hardship experienced at any time during the three months prior to the 42-month interview, because of lack of money (%)				
Sample member did not buy food	23.6	20.5	3.1	0.113
Sample member, partner, or children did not see dentist when needed	23.3	21.6	1.8	0.359
Sample member, partner, or children did not see doctor when needed	8.7	6.4	2.2 *	0.081
Utilities were turned off	4.0	3.1	0.9	0.321
Phone was turned off	22.8	21.8	1.0	0.592
Children were ever without food considered necessary (%)	4.9	4.6	0.2	0.829
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

impacts on these outcomes. Few experimental/control differences were statistically significant, but the differences generally favored the control group. Experimentals were more likely to report occasionally not buying food because of lack of money (23.6 percent versus 20.5 percent for controls), and they were more likely to report not seeing a doctor because of financial problems (8.7 versus 6.4 percent). Nearly a quarter of both groups reported that they could not afford to see a dentist or were without telephone service because of a lack of money.

V. Conclusion

The findings presented in this chapter are not encouraging. It appears that New Chance did not succeed in improving employment outcomes for the young women it served. The program also failed to reduce sample members' dependency on welfare, and it did not increase their income. In a nutshell, there appear to be two explanations for these disappointing findings: (1) that controls did better than expected and (2) that the receipt of education services among experimentals often was limited to adult education (ABE/GED), which in itself did not improve their long-run employment outcomes. On the positive side, nonexperimental analyses suggested a substantial payoff for skills training, college, and receipt of a training certificate or trade license. Unfortunately, the experimental effects on participation in skills training and college were limited, especially compared with the substantial program effects on adult education and GED receipt.

The fact that controls did better than expected implies that disadvantaged young mothers like those served by New Chance can and do find education and training opportunities on their own. They also find jobs on their own, although these jobs are often of short duration and generally do not pay enough to result in a permanent exit from welfare. Also, many potential job opportunities are rejected, and many jobs are lost because of child care problems and other practical personal issues like transportation and health problems. Rather than focusing primarily on remedial education and training, programs like New Chance may have to become actively involved in the work life of program participants after they leave the program.

Chapter 8

Parenting, Child Care, and Child Development

I. Introduction

A. Background

In its design, the New Chance program was given an explicit two-generational focus in an attempt to improve outcomes for children as well as for their mothers. The program was designed to affect child development outcomes both directly, by providing services to the children, and indirectly, through services provided to the mothers participating in New Chance.

One service targeted directly at the children was developmentally appropriate child care available to New Chance enrollees while they participated in program activities. More than half the New Chance sites had on-site child care. These on-site facilities were shaped in part by guidelines developed in collaboration with child development scholars. The provision of high-quality child care was explicitly included in the New Chance model because such care had been found to have beneficial effects on the development of disadvantaged children (see, for example, Burchinal, Lee, and Ramey, 1989; Caughey, DiPietro, and Strobino, 1994; Martin, Ramey, and Ramey, 1990; Ramey and Campbell, 1994).

The second direct service was pediatric care, usually provided through referral to health-care providers. Pediatric services were intended to enhance the health and health-care utilization of participants' children, who because of their poverty were at higher-than-average risk of medical problems.

New Chance also sought to improve children's outcomes indirectly, by offering parenting instruction and support to the mothers enrolled in the program. The program's parenting component was designed both to strengthen participants' parenting skills (for example, by helping them to a better understanding of the developmental phases of childhood and of effective methods of disciplining children) and also to support and encourage them in coping with the stresses of parenthood. Good parenting skills have repeatedly been found to predict both positive socioemotional outcomes (Bank et al., 1993; Denham, Renwick, and Holt, 1991; Hubbs-Tait et al., 1994) and positive cognitive and school performance outcomes among children (Coates and Lewis, 1984; Estrada et al., 1987; McGowan and Johnson, 1984). Moreover, evidence from Project Redirection has indicated that comprehensive programs offering parenting education to disadvantaged young mothers can have positive effects on the quality of the home environment and on children's development (Polit, Quint, and Riccio, 1988). As with the child care component, child development experts contributed to the design of the program's parenting component.

It was also expected that if New Chance improved the educational attainment and financial circumstances of the young mothers in the sample, their children would be among the primary beneficiaries. There is considerable evidence that increased levels of maternal education and family income are positively associated with children's cognitive and social development (Entwistle and Alexander, 1990; Garcia-Coll, 1990; Patterson, Kupersmidt, and Vaden, 1990; Takeuchi, Williams, and Adair, 1991). In addition, it was expected that if the program had a positive effect on the mother's emotional well-being and psychological resources, the quality of the mother-child relationship would be enhanced; mental health variables repeatedly have been found to be related to mother-child relations and to children's socioemotional development in poor and minority families (Klebanov, Brooks-Gunn, and Duncan, 1994; McLoyd, 1990; Simons et al., 1993

In summary, New Chance was designed to have beneficial effects on the development of young children, and therefore it is important to evaluate whether such effects occurred. This chapter first examines program impacts on several variables indirectly related to children's development, specifically on various aspects of parenting, including parental stress and the overall quality of the home environment. The chapter then examines child care use during the follow-up period. Finally, impacts on child health and development are analyzed.[1]

B. The Focal Child

Because of resource constraints, it was not possible to examine program impacts on all children of the sample members. Instead, one child in each family was identified as the "focal child," to whom all questions relating to a specific child would apply. In scheduling the follow-up interviews, interviewers asked mothers to have the focal child present. In this chapter, all references to the sample members' children concern the focal child, unless it is otherwise stated.

The majority of women in the sample (65 percent) had only one child at baseline, who was automatically designated as the focal child. When there were two or more children at baseline, a focal child was randomly selected from among them.[2] Information about a focal child was obtained for 96 percent of the women who completed follow-up interviews.

At the time of the final interview, the focal children were between 3½ and 10 years old; their average age was just under 5 years, or about 18 months when their mothers entered the study. The majority of these children (82 percent) were firstborns, as would be expected since most of the young women had only one child at baseline. There were slightly more male than female focal children in the sample (52 percent versus 48 percent, respectively).

C. Preview of the Findings

At the time of the 18-month follow-up, the children in the experimental group were in home environments where there was somewhat more emotional support than in the control group and where the mothers were less likely to express disfavor with the parenting role. By the time of the final interview, however, these group differences had disappeared. Overall, the quality of the home environment was similar in the two groups at both points in time. By the final interview, women in the experimental group reported significantly higher levels of parenting stress in relation to the focal child (especially if the child was male) than did their counterparts in the control group.

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[1] More detailed information about the parenting and child development measures is available in Polit, 1996a.

[2] If the randomly selected focal child was not living with the mother at the time of the follow-up interview or was otherwise unable to participate in the interview, the interviewer was allowed to substitute another child as the focal child, but only if the alternate child had been born before random assignment and was currently living with the mother. Substitutions occurred in fewer than one percent of the interviews.

In the early months after random assignment, focal children in the experimental group were exposed to considerably different child care experiences from those of their control group counterparts. Even though the majority of children in the control group were in a child care arrangement during the first follow-up period, they were likely to be in child care for fewer months and were primarily cared for by relatives. Children in the experimental group were likely to be in nonmaternal child care for longer periods of time and were substantially more likely to be cared for in day care centers; they were also more likely to have entered regular nonmaternal child care before age 1. Between the 18-month and 42-month interviews, the child care experiences of the focal children in the two groups were similar, but by the end of the study a higher percentage of children in the experimental group had spent some time in a day care center than children in the control group. Children in the experimental group also experienced more changes in their child care arrangements than children in the control group.

There were few program effects on measures of child health included in the follow-up surveys. Maternal ratings of overall health, days spent in bed for illness, and number of hospitalizations were similar for focal children in the experimental and control groups. However, there was a small impact on reported injuries, poisonings, and accidents that required medical attention. This impact did not increase rates of hospitalization and may have been related to greater access to health care among experimentals. The two groups were similar with respect to measures of cognitive development at the 42-month interview, including scores on a standardized measure of school readiness and teacher ratings of academic progress. There were, however, statistically significant differences between experimentals and controls on several measures of socioemotional development, particularly measures that involved maternal reports of the child's behavior. Children in the experimental group were described by their mothers as having more behavior problems (and fewer positive behaviors) than those in the control group. Teachers generally described the two groups as being similar. The impacts on mother-reported behavior problems were concentrated in young families in which the mother was at risk of depression upon her entry into the study. Impacts were also unfavorable for young women who faced multiple risks at baseline.

Several factors are likely to have contributed to the less favorable developmental outcomes among children whose mothers were at risk of depression or faced multiple barriers at baseline. These include subsequent higher levels of stress and depression among the mothers as reported in the 42-month interview, both of which may have affected maternal reports on the children's behavior, as well as the children's actual behavior itself, and greater use of day care centers among these children (possibly confounded with a higher number of different child care arrangements).

II. Parenting and the Home Environment

As was indicated in Chapter 1, research has fairly consistently shown that teenage mothers tend to be less competent parents and tend to raise their children in less favorable home environments than do women who delay childbearing. These findings are consistent with evidence that the children of young mothers have less promising developmental outcomes than other children (Brooks-Gunn and Furstenberg, 1986; Cooley and Unger, 1991; Kinard and Klerman, 1983; Moore and Snyder, 1991). Since poverty is also associated with children's cognitive and behavioral problems, children born to young women who are poor, like those in the New Chance research sample, are especially at risk for developmental difficulties.

The parenting component in New Chance was designed to promote positive parenting practices, to foster the development of healthy mother-child relationships, and to reduce the stresses associated with parenthood — all with the aim of stimulating the early development of the participants' children. The program's effects on parenting and the home environment were assessed at both the 18-month and 42-month interviews.

A. Parenting Measures

The measures of parenting used in the follow-up interviews were based largely on maternal self-report. Such reports are not the ideal means of measuring parenting behaviors, but in-depth observation of mother-child interactions by child development experts was not a viable alternative for the entire sample.[3]

1. **Home Observation for Measurement of the Environment.** In both the 18-month and 42-month interviews, the quality of the home environment was measured with a shortened version of the Home Observation for Measurement of the Environment (HOME) scale (Caldwell and Bradley, 1984). The HOME scale, a widely used measure of home environmental processes that have been shown to be related to children's development, relies heavily on maternal report but also includes several observational items. The interviewers' observations primarily concern ratings of the quality of the physical environment, such as its cleanliness and safety, and specific aspects of mother-child interactions, such as whether the mother spanked the focal child during the interview.

The HOME scale used in the 18-month interview was the version used in the National Longitudinal Survey of Youth (Baker and Mott, 1989), referred to as the HOME-SF (short form). For the purpose of the impact analyses, adaptations to the HOME-SF were made because of concern about the scale's reliability in this sample, but the scale was also scored according to the NLSY procedures so that the home environments of the New Chance sample could be compared with those of a national sample.[4] Further adaptations were made to enhance the reliability of the 42-month version of the HOME.

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[3] A supplementary observational study was conducted with a subsample of 290 New Chance sample members, however. This embedded study involved making videotapes of the mother and child during 30 to 40 minutes of interaction. The videotapes were subsequently viewed and coded by two teams of child development scholars. For the separate monograph describing the results of this study, see Zaslow and Eldred (eds.), forthcoming.

In the version of the HOME used for the impact analyses at both the 18-month and 42-month points, in addition to a total HOME scale the following subscales were created:

- *Cognitive Stimulation,* evaluating the extent to which the home environment included cognitively stimulating resources — such as books and toys — and how much the adults in the home engaged the child in stimulating activities, such as reading to the child

- *Emotional Support,* evaluating the warmth and supportiveness of the mother's interactions with the child

- *Physical Environment,* evaluating the degree to which the interior and exterior of the home were clean, safe, and pleasant

- *Harsh Discipline,* evaluating the methods the mother used to punish the child and whether they were physical or harsh

The HOME has three alternative forms, corresponding to the age of the child.[5] All three forms were used in the 18-month survey, when the children's ages ranged from under 2 to over 7 years old. Only the two forms for older children (the preschool-age and school-age forms) were needed in the 42-month interview. To analyze impacts on the HOME scores for the entire New Chance sample (rather than conducting separate analyses for children of different ages), the raw HOME scores were converted to standard scores that had a mean of 100 and a standard deviation of 15 within each age group.

2. Parenting Stress Scale. In addition to the HOME, a measure of parenting stress was included in both follow-up surveys. The Parenting Stress Scale is an eight-item maternal-report scale that measures the degree of stress that the mother perceives as a parent, especially in relation to her interactions with the focal child. An example of an item on the Parenting Stress Scale is "I often feel angry with my child." Theoretically, the scores could range from a low of 0 (total absence of stress, or complete enjoyment of the parenting role) to a high of 80 (extreme parental stress and aggravation). Actual scores ranged from 0 to 71 at 18 months and from 0 to 80 at 42 months; over 80 percent of the mothers scored below the theoretical midpoint of 40 at both interviews.[6]

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[4] Adaptations to the HOME scale were made at the 18-month point after it was determined that the NLSY scoring of the scale yielded very low reliability coefficients; further changes were made for the 42-month survey. Presumably, the much lower reliabilities than were reported for the NLSY sample reflect the greater homogeneity of the New Chance sample. The adaptations made for this study involved adding several items to the HOME and using a trichotomous scoring procedure that made finer discriminations than the traditional dichotomous scoring of the HOME. The adaptations to the 42-month HOME were made in consultation with Dr. Robert Bradley, who was one of the developers of the original HOME scale.

[5] The three forms included an infant form for children under age 3, a preschool form for children age 3 to 6, and an elementary school-age form for children age 6 and older. The internal consistency reliability for the total HOME scale ranged from .70 to .76 for the three forms at the 18-month point and between .81 and .82 for the two forms used at the 42-month point.

The Parenting Stress Scale had two subscales — one five-item subscale measuring the mother's aggravation with the focal child and a three-item subscale capturing her disliking of the parental role.[7]

B. Aggregate Program Impacts on Parenting and the Home Environment

Table 8.1 presents impact information on the subscale and total HOME scores for the full New Chance sample. The average total HOME scores were virtually identical (about 100) for the two groups, reflecting similar home environments overall for experimental and control children at both survey points.[8] The two groups also had comparable average scores (all near 100) on three of the HOME subscales — the Cognitive Stimulation, Physical Environment, and Harsh Discipline subscales.

On the Emotional Support subscale of the HOME at the 18-month interview, however, the experimental group had a somewhat higher (that is, more favorable) mean score (100.4) than the control group (99.1). The Emotional Support subscale, unlike the other HOME subscales, consisted almost exclusively of interviewer observation rather than maternal reports. For example, interviewers indicated whether the mother's voice conveyed positive feelings about the focal child and whether she caressed, hugged, or kissed the child at least once during the interview. Thus, the experimental and control groups differed on a measure that was less likely than other subscales to be influenced by social desirability and other reporting biases.[9] Nevertheless, despite the impact on an important subscale of the HOME, the magnitude of the group difference on the Emotional Support subscale was quite small, and the group difference was no longer statistically significant at the 42-month point.

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[6] The internal consistency reliability of this scale for the New Chance sample was .70 at the 18-month interview and .76 at the 42-month interview.

[7] An example of an item on the Aggravation subscale is "My child seems to be much harder to care for than most." An example of an item on the Dislike of Parenting Role subscale is "I feel trapped by my responsibilities as a parent."

[8] Total HOME scores at the 42-month point were more favorable when the mothers had a diploma or GED, a finding consistent with a very abundant literature ($r = .24$, $p = .0001$). HOME scores were higher among women who had been employed at some point in the two years before the final interview ($r = .15$, $p < .0001$). HOME scores also were correlated with living arrangements. In general, the greater the number of children in the household, the lower the HOME scores, and the greater the number of adults, the higher the HOME scores (the correlation between HOME scores and a variable indicating the child-to-adult ratio was -.23). Both living with a partner ($r = .11$) and living with a parent ($r = .07$) were associated with better HOME scores, while living alone with children was associated with worse HOME scores ($r = -.13$).

[9] The risk of interviewer bias seems minimal. Interviewers knew whether respondents were in the experimental or control group, but they knew little about the actual intervention or the program goals. Interviewers also had no contact with program staff, nor were any interviews conducted at a program site. Thus, interviewers had no particular reason to be biased in their observations of the mothers interacting with their children.

Table 8.1

Impacts of New Chance on Parenting Outcomes at 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Average scores on HOME scale at 18-month follow-up				
Total HOME scale	100.0	99.8	0.2	0.739
Cognitive Stimulation subscale	100.1	100.2	-0.1	0.911
Emotional Support subscale	100.4	99.1	1.3 *	0.072
Physical Environment subscale	99.5	100.3	-0.8	0.257
Harsh Discipline subscale	100.2	99.3	0.9	0.253
Average scores on HOME scale at 42-month follow-up				
Total HOME scale	100.1	100.0	0.1	0.816
Cognitive Stimulation subscale	100.2	100.1	0.1	0.866
Emotional Support subscale	100.3	99.7	0.6	0.408
Physical Environment subscale	100.0	99.9	0.1	0.928
Harsh Discipline subscale	99.9	100.6	-0.7	0.336
Average scores on Parenting Stress Scale at 18-month follow-up[b]				
Total Parenting Stress Scale	28.0	28.1	-0.1	0.911
Aggravation with Child subscale	14.8	14.3	0.5	0.263
Dislike of Parenting Role subscale	13.0	13.7	-0.6 *	0.079
Average scores on Parenting Stress Scale at 42-month follow-up[b]				
Total Parenting Stress Scale	26.4	24.6	1.7 **	0.013
Aggravation with Child subscale	14.9	13.2	1.7 ***	0.000
Dislike of Parenting Role subscale	11.5	11.4	0.1	0.881
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) scale (first administered in the National Longitudinal Survey of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Parenting Stress Scale is an eight-item scale developed for the New Chance study, with items adapted primarily from Abidin's Parenting Stress Index. Scores can range from zero (no stress) to 80 (extreme stress). The two subscales were derived on the basis of a factor analysis.

Table 8.1 also shows the program's impacts on the Parenting Stress Scale and subscales. The two groups were similar on the total scale at the 18-month interview, as well as on the Aggravation subscale, but the experimental group scored significantly lower (that is, more favorably) on the subscale capturing dislike of the parenting role at the first follow-up interview. The situation was different at the final follow-up, however, when mothers in the experimental group scored higher (less favorably) on the total Parenting Stress Scale (mean = 26.4) than mothers in the control group (mean = 24.6). The differences on the total scale are attributable primarily to group differences on the Aggravation subscale; experimental group mothers reported higher levels of aggravation in relation to their children than control group mothers (mean scores of 14.9 and 13.2, respectively). The two groups had comparable scores on the Dislike of Parenting Role subscale at the final interview.

It might be noted that scores on the Parenting Stress Scale were correlated with total HOME scores (r = -.24) and, especially, with the Harsh Discipline subscale of the HOME (r = -.29). Mothers with higher scores on the CES-D (depression) scale tended to be raising children in less favorable home environments (r = -.21 with total HOME scores) and tended to be more stressed as parents (r = .37 with scores on the Parenting Stress Scale). Finally, it is worth noting that Zaslow and Eldred (1997) found statistically significant program effects on two observational measures of parenting for a not quite representative subsample of 290 New Chance sample members.

C. Subgroup and Site Impacts on Parenting and the Home Environment

This section examines the impact of New Chance on parenting measures for various subgroups of sample members, including not only those defined by maternal characteristics at baseline, as in previous chapters, but subgroups based on the focal child's gender and age. For the age subgroups, the sample was divided into two groups: children who were younger than 1½ years old (18 months) versus those who were 1½ years old or older at random assignment. At the time of the 42-month interview, therefore, the younger group was almost entirely under 5 years of age (and mostly not in school) and the older group was 5 or older (and mostly in school).[10]

At the 18-month interview, the scores on the Emotional Support subscale of the HOME were higher for the experimental group than for the control group for most of the subgroups examined, with differences reaching statistical significance for several subgroups (not shown). One unexpected finding was that the program impact on this scale was statistically significant only when the focal child was a girl. Among female focal children, the mean Emotional Support subscale score for experimentals was 101.6, compared with 98.2 for controls; the impact for boys was small and negative (and statistically significantly different from the impact on girls). Moreover, although the overall experimental/control group difference on the total 18-month HOME score was not statistically significant, this score was significantly higher among experimentals (mean = 101.4) than among controls (mean = 99.3) when the focal child was female ($p < .05$, not shown).

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[10] At the 42-month follow-up, the average age of the children in the younger subgroup was 3.8 years; 92 percent of these children were under age 5 at the final interview. The average age of the children in the older subgroup was 6.0 years; 99.9 percent of these children were older than 5 years old at the final interview, and 82.9 percent of them were in school.

Table 8.2 shows that at the 42-month interview, there were two subgroups for which New Chance had a statistically significant impact on the total HOME score. First, among mothers who were not at risk of depression at the time of random assignment, those in the experimental group had more favorable scores on the HOME than those in the control group. Among those at risk of depression at baseline, the impact was reversed and was almost statistically significant. The difference in impacts across the depression subgroups was statistically significant. There was also a favorable impact on the 42-month HOME score for the youngest sample members (those who were 16 or 17 years old at random assignment).[11] Finally, it should be noted that at the 42-month point, the favorable program impact on HOME scores for the girls had disappeared.

With respect to scores on the Parenting Stress Scale at the final interview, the unfavorable program impact was observed in many subgroups and was more likely to be statistically significant for the more disadvantaged ones, as shown in Table 8.3. For example, the impact was most pronounced among women who had been at very high risk of depression at baseline (experimental mean = 30.2, control mean = 26.5). Other disadvantaged subgroups for which there were impacts included those who at baseline had been out of school two or more years, had reading levels at or below 7th grade, or had completed 10th grade or less. There was an unfavorable impact on Parenting Stress for older but not for younger children and for male children but not for female children.

At the site level, scores on the Parenting Stress Scale at the 42-month interview were higher among experimentals than among controls in three sites: the Bronx, Detroit, and Philadelphia. The difference in impacts across sites was almost statistically significant (p = .113, not shown).

In summary, the program appears to have had a modest favorable impact on several measures related to parenting at the time of the first follow-up interview. The home environments of the two groups were similar at the final interview, however, and mothers in the experimental group — especially those who were at very high risk of depression when they applied to New Chance — were significantly more aggravated with their children than mothers in the control group.

III. Child Care and Schooling

Child care was an integral component of the New Chance model and was viewed as fulfilling two roles. First, child care that was free, reliable, and convenient was considered essential to the New Chance participants, to enable them to participate regularly in the full schedule of New Chance activities. Second, in keeping with the two-generational focus of New Chance, child care was regarded as an important mechanism through which the child development needs of participants' children could be addressed, given the evidence that high-quality child care programs can have positive effects on the development of disadvantaged children. Thus, programs were encouraged to offer on-site child care that was sensitive to the developmental milestones and

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[11] This subgroup impact is consistent with the findings in Project Redirection (Polit, Quint, and Riccio, 1988), which found favorable impacts on the HOME five years after baseline. The similarity of findings arises from the fact that Project Redirection was an intervention aimed at teen mothers age 17 or younger.

Table 8.2

Impacts of New Chance on Total Standardized HOME Scale Scores at 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Total Standardized HOME Scale Score		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.202
16-17	402	100.0	97.3	2.6 *	0.081		
18-19	997	100.6	101.1	-0.5	0.621		
20-22	678	99.1	99.2	-0.1	0.919		
Ethnicity						---	0.619
Black, non-Hispanic	1,087	98.3	97.6	0.7	0.401		
Hispanic	474	101.6	102.3	-0.7	0.614		
White or other	515	102.7	103.0	-0.4	0.795		
Highest grade completed						2.1	0.127
10th or below	1,391	99.6	98.8	0.8	0.295		
11th or above	684	100.8	102.1	-1.3	0.258		
Interval since last attended regular high school						-1.0	0.435
More than 2 years	1,093	99.7	100.1	-0.4	0.651		
2 years or less	927	100.5	99.8	0.6	0.517		
TABE reading test score (grade equivalent)[c]						---	0.919
Below 6th grade	433	97.9	98.3	-0.4	0.774		
6th or 7th grade	492	98.7	97.8	0.9	0.501		
8th or 9th grade	566	100.8	100.5	0.3	0.797		
10th grade or above	583	102.0	102.0	0.0	0.975		
Family received AFDC when sample member was growing up						---	0.215
Always	341	98.4	96.4	2.0	0.219		
Sometimes	970	100.3	99.5	0.8	0.406		
Never	749	100.5	101.6	-1.1	0.298		
Ever employed						1.4	0.385
Yes	1,646	100.1	100.2	-0.1	0.875		
No	433	99.9	98.7	1.3	0.369		
CES-D (depression) Scale[d]						--- **	0.031
0-15 (not at risk)	967	101.2	99.2	2.0 **	0.040		
16-23 (at some risk)	525	99.5	101.5	-1.9	0.129		
24-60 (at high risk)	582	98.4	99.3	-0.9	0.479		
Multiple risk score[e]						---	0.486
Low	871	101.3	100.5	0.9	0.385		
Moderate	618	99.5	100.5	-1.0	0.405		
High	525	98.6	98.3	0.3	0.816		

(continued)

Table 8.2 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Total Standardized HOME Scale Scores: Experimentals	Total Standardized HOME Scale Scores: Controls	Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
Focal child's gender[f]						0.3	0.810
Girl	952	100.8	100.4	0.3	0.710		
Boy	1,037	99.4	99.4	0.0	0.970		
Focal child's age at baseline[f]						-1.6	0.208
Less than 18 months	1,172	99.4	100.0	-0.6	0.491		
18 months or older	902	100.9	99.8	1.1	0.283		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: A modified version of the short form of the Home Observation for Measurement of the Environment (HOME) scale (first administered in the National Longitudinal Study of Youth) was administered. Scores here were age-standardized to have a mean of 100 and a standard deviation of 15.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as ***= 1 percent; **= 5 percent; *= 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

[f] For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Table 8.3

Impacts of New Chance on Parenting Stress Scale Scores at 42 Months After Random Assignment, for Selected Subgroups

Characteristic and Subgroup at Random Assignment	Sample Size	Parenting Stress Scale Score: Experimentals	Parenting Stress Scale Score: Controls	Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
Age (years)						--- *	0.051
16-17	402	26.4	26.8	-0.4	0.822		
18-19	997	25.9	25.0	0.9	0.357		
20-22	678	27.3	23.2	4.1 ***	0.001		
Ethnicity						---	0.362
Black, non-Hispanic	1,087	27.7	25.2	2.5 ***	0.007		
Hispanic	474	25.0	23.9	1.2	0.435		
White or other	515	24.8	24.7	0.2	0.922		
Highest grade completed						0.3	0.848
10th or below	1,391	26.4	24.6	1.8 **	0.032		
11th or above	684	26.4	24.9	1.6	0.199		
Interval since last attended regular high school						3.1 **	0.028
More than 2 years	1,093	26.9	23.7	3.2 ***	0.001		
2 years or less	927	25.9	25.9	0.1	0.931		
TABE reading test score (grade equivalent)[c]						---	0.302
Below 6th grade	433	27.4	24.2	3.1 **	0.040		
6th or 7th grade	492	27.4	24.6	2.8 **	0.049		
8th or 9th grade	566	24.5	24.8	-0.2	0.864		
10th grade or above	583	26.8	25.2	1.5	0.254		
Family received AFDC when sample member was growing up						--- *	0.071
Always	341	26.1	25.1	1.1	0.551		
Sometimes	970	27.1	23.7	3.4 ***	0.001		
Never	749	25.9	26.0	-0.1	0.929		
Ever employed						-0.4	0.818
Yes	1,646	26.5	24.7	1.8 **	0.022		
No	433	26.3	24.9	1.4	0.349		
CES-D (depression) Scale[d]						---	0.141
0-15 (not at risk)	967	24.0	23.5	0.4	0.681		
16-23 (at some risk)	525	26.9	25.1	1.8	0.194		
24-60 (at high risk)	582	30.2	26.5	3.7 ***	0.005		
Multiple risk score[e]						--- **	0.031
Low	871	25.4	25.8	-0.4	0.74		
Moderate	618	27.7	24.7	3.0 **	0.02		
High	525	26.8	23.0	3.8 ***	0.01		

(continued)

Table 8.3 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	Parenting Stress Scale Score		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Focal child's gender[f]						-1.7	0.237
Girl	952	25.0	24.3	0.7	0.479		
Boy	1,037	27.7	25.3	2.4 **	0.015		
Focal child's age at baseline[f]						-1.4	0.306
Less than 18 months	1,172	26.6	25.5	1.1	0.239		
18 months or older	902	26.3	23.7	2.5 **	0.016		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: The Parenting Stress Scale is an eight-item scale developed for the New Chance study, with items adapted primarily from Abidin's Parenting Stress Index. Scores can range from zero (no stress) to 80 (extreme stress). The two subscales were derived on the basis of a factor analysis.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

[f] For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

emotional and cognitive needs of the children. In sites that were unable to offer on-site care, or when the mothers themselves did not want to use on-site care, program staff were expected to assist the mothers in finding a no-cost child care arrangement that was compatible with full-time program participation.

On-site child care was available to New Chance participants in 12 of the 16 sites, although in two of these sites the child care facility provided only temporary or drop-in services for emergency use and in another site the on-site care was used by only a very small number of New Chance participants, because slots were not reserved for them. Sites without full-time on-site centers relied on linkages with child care centers that typically were located within a fairly short distance of the program.

As was noted in Chapter 4, a special study of New Chance child care facilities was undertaken (Fink, 1994). In this study, child care quality was assessed through both measures derived from staff interviews (for example, group size and child/staff ratios) and observational ratings of the quality of care. The findings indicated that the child care provided in most of the New Chance child care facilities was congruent with child care guidelines propounded by experts and that the facilities were generally of higher quality than the typical child care center serving primarily low-income families.

A. Aggregate Program Impacts on Child Care and Schooling

Given expected program effects on the mothers' involvement in education and training programs and the program staff's mandate to assist participants with child care arrangements, it was anticipated that child care patterns in the two groups would differ with respect to the type of arrangements and the total amount of child care used in the follow-up period.

Table 8.4 presents information on the sample members' use of child care arrangements for the focal child after random assignment. This table indicates that even in the absence of New Chance, most of the mothers used some type of regular child care in the early months after baseline; 85.0 percent of the children in the control group were in a child care arrangement at some point between baseline and the 18-month interview. This high usage of child care is consistent with the fact that a high percentage of controls participated in employment and training activities or held a job during the early follow-up period. Even more of the mothers in the experimental group (95.2 percent), however, had made a child care arrangement for the focal child, and this impact was statistically significant.[12] Experimental group members' children were also more likely than their control group counterparts to have entered a regular child care arrangement prior to their first birthday (48.4 percent versus 41.0 percent, respectively).

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[12] Notably, the rate of nonmaternal child care is extremely high in both groups in comparison with rates reported in other studies of disadvantaged young mothers. For example, in the Teenage Parent Demonstration, which involved *mandatory* participation in various activities for those in the enhanced service group, the percentage of controls using child care during a two-year follow-up period ranged from 55 percent in one site to 70 percent in another, and the percentage of experimentals using child care ranged from 69 to 80 percent (Maynard, Nicholason, and Rangarajan, 1993).

Table 8.4

Impacts of New Chance on Child Care and Schooling for Focal Child at or Within 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Ever in any child care arrangement (%)[b]				
at 18-month follow-up	95.2	85.0	10.2 ***	0.000
before age 1	48.4	41.0	7.4 ***	0.001
Average number of child care arrangements between random assignment and 18-month follow-up	2.2	1.8	0.4 ***	0.000
Average number of child care arrangements between random assignment and 18-month follow-up for those with child care[c]	2.9	2.6	0.3 ***	0.007
Between random assignment and 18-month follow-up, use of/attendance in: (%)				
Day care center/preschool	63.9	31.3	32.6 ***	0.000
Family day care/unrelated baby-sitter	27.2	24.1	3.1	0.141
Regular care by grandparent	44.3	43.7	0.6	0.793
Between 18- and 42-month follow-up, use of/attendance in: (%)				
Day care center/preschool	69.1	51.3	17.8 ***	0.000
Head Start program	39.0	42.2	-3.3	0.161
At 18-month follow-up, use of/attendance in: (%)				
Any child care[b]	51.1	52.6	-1.5	0.551
Day care center/preschool	18.0	13.5	4.5 **	0.015
Family day care/unrelated baby-sitter	8.8	8.3	0.4	0.765
Regular care by grandparent	16.7	18.5	-1.9	0.326
Head Start program	4.7	4.7	0.0	0.996
School	5.7	6.3	-0.6	0.572
At 42-month follow-up, use of/attendance in: (%)				
Any child care[b]	81.4	83.1	-1.7	0.338
Day care center/preschool	12.7	11.5	1.2	0.442
Family day care/unrelated baby-sitter	15.1	14.4	0.7	0.692
Regular care by grandparent	30.3	28.4	2.0	0.405
Head Start program	11.0	11.5	-0.5	0.736
School	38.5	39.6	-1.1	0.585
Average number of months in Head Start program[d]	3.7	3.9	-0.2	0.610
Sample size	1,401	678		

(continued)

Table 8.4 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] Any child care arrangement includes kindergarten or elementary school, extended day program, summer program or day camp, Head Start, day care center, nursery school, preschool, and grandparent, child's father, or other relative.

[c] This comparison includes only 923 sample members who had any child care from random assignment to 18-month follow-up.

[d] The number of months is the average number of months between random assignment and follow-up during which the child was in Head Start. The average includes mothers who never used Head Start and for whom the number of months would therefore have been zero.

During the first 18 months, experimentals not only were more likely to use child care but also used more separate arrangements. While children in the control group experienced 1.8 arrangements on average, the children of experimentals were in 2.2 arrangements; among sample members who used child care, experimentals used 2.9 arrangements and controls used 2.6. These differences were statistically significant.

Group differences in child care use were especially pronounced with respect to the use of a day care center or preschool in the first 18 months after random assignment. As is shown in Table 8.4, 63.9 percent of the experimentals and 31.3 percent of the controls were enrolled in a day care center or preschool program between baseline and the 18-month interview. New Chance had an impact not only on the incidence of day care center use but also on duration. By the time of the 18-month interview, experimental group children had spent an average of 4.8 months in a day care center or preschool program, compared with an average of 2.4 months for control group children, an impact that was highly significant ($p < .01$, not shown). These numbers are the averages for all mothers, including those who did not use a day care center and for whom the number of months was therefore zero.[13]

In addition to the impact on the use of day care centers, focal children in the experimental group (27.2 percent) were more likely than those in the control group (24.1 percent) to have been cared for in a family day care home or in a paid babysitting arrangement during the first 18 months of follow-up. Among controls, the most frequently used type of nonmaternal child care during the first 18 months after random assignment was care by a grandparent, reported by 43.7 percent of the controls, and a comparable percentage of mothers in the experimental group had obtained child care from a grandparent during this same period. The two groups also did not show differences in the use of other categories of care, such as care by the mother's husband or sister (not shown). Thus, while participation in the program did not reduce the percentage of children who had been cared for by relatives during the follow-up period, New Chance had a substantial impact on the use of nonrelative care for the focal child. This finding is consistent with expectations and undoubtedly reflects the use of on-site or program-referred child care centers by many mothers in the experimental group.[14]

Program effects on the use of child care were especially large during the first several months after random assignment and, for many sample members, were not very long-lived. For example, differences in the use of "market" child care (day care centers, family day care homes, paid babysitters, and so on) peaked in the second month after baseline and declined steadily thereafter. Figure 8.1 shows the use of market child care for the first 18 months after random assignment; 53 percent of the experimentals and 13 percent of the controls were using market care in the second month post-baseline.

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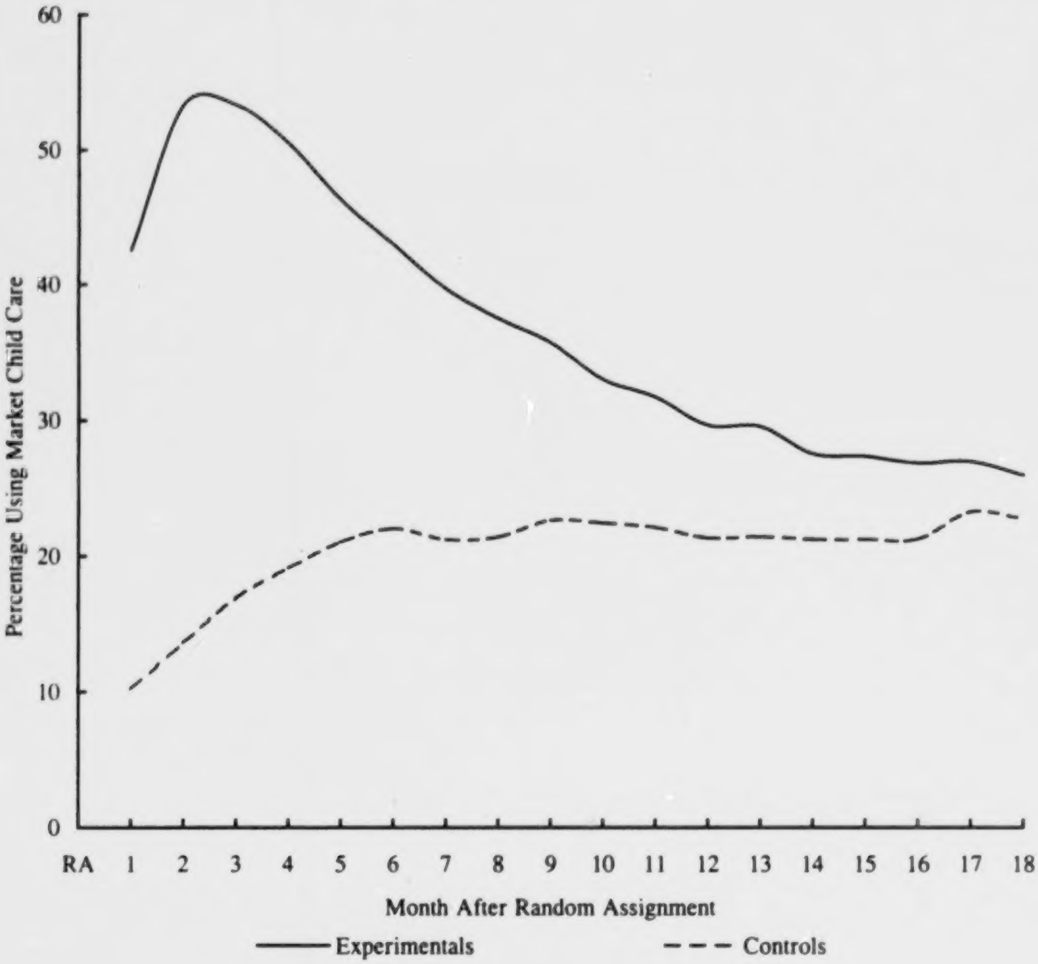
[13] Among the focal children ever in a day care center, the mean number of months in such care between random assignment and the 18-month interview was 7.7 for experimentals' children and 7.0 months for controls' children.

[14] Over half (52.9 percent) of the experimental group mothers said that they had used child care that was provided directly by the New Chance program. In several sites (Allentown, Detroit, Lexington, and Salem), over 80 percent of the mothers had used on-site care.

Figure 8.1

Use of Market Child Care by New Chance Sample Members Within 42 Months After Random Assignment



SOURCE: MDRC calculations from New Chance survey data. See Appendix Table G.15 for data corresponding to figure.

NOTE: Market child care includes care at a day care center, a preschool, a family day care home or by a paid babysitter.

As is indicated in Table 8.4, group differences in child care usage had virtually disappeared by the time of the first follow-up interview. Comparable percentages of children in the experimental group (51.1 percent) and control group (52.6 percent) were in a regular child arrangement or in school at that point. The *type* of child care used also was similar for experimentals and controls at 18 months after random assignment. Comparable percentages of children in both groups were in a day care center, in family day care, being cared for by grandparents, were in Head Start, or were in school at the 18-month interview (although experimentals were still somewhat more likely to use center-based day care). These similarities persisted up until the final interview. At the 18-month point, the most common arrangements for both groups were day care centers and regular care by a grandparent. Experimentals were significantly more likely to use a day care center or preschool (18.0 percent of experimentals and 13.5 percent for controls). By the time of the final interview, school had become the most common arrangement; nearly 40 percent of the focal children in both groups were in school.

Table 8.4 indicates that over time controls increased their use of day care centers to a much greater degree than experimentals. Controls increased their use of day care centers from 31.3 percent between baseline and 18 months to 51.3 percent between 18 and 42 months, compared with an increase from 63.9 percent to 69.1 percent for experimentals. Nevertheless, the group differences in ever having used a day care center remained significant at the end of the study.

Group differences with respect to having been enrolled in a Head Start program by the final interview were not statistically significant; 39.0 percent of the children in the experimental group had enrolled in Head Start, compared with 42.2 percent of the controls. On average, children in both research groups had spent just under 4 months in Head Start (equivalent to a full year for those who were enrolled).

In summary, the child care experiences of the focal children in the two groups were quite different in the months immediately after random assignment. Focal children in the experimental group were especially likely to have spent some time with child care providers who were not related to them. Moreover, the children in the experimental group were more likely to have entered a regular child care arrangement as infants. The differences in child care use were of relatively short duration, however, and by the time of the first interview, child care use patterns in the two groups were comparable.

B. Subgroup and Site Impacts on Child Care and Schooling

Program effects on the use of a day care center or preschool in the period from baseline to the 18-month interview were statistically significant for a wide range of subgroups (not shown). The size of the program effects was consistently large, ranging from a low of 25.8 percentage points (for women who were not at risk of depression at baseline), to a high of 35.8 percentage points (for women who were at high risk of depression at baseline). By the 42-month point, the impacts on having ever been in a day care center or preschool were still substantial across subgroups (not shown); impacts ranged from 11.8 percentage points (women who were 16–17 at baseline) to 24.4 percentage points (women reading at the 8th- or 9th-grade level at baseline).

Looking across sites, it appears that at every site a higher percentage of experimentals than controls had used a day care center or preschool between random assignment and the 18-month interview. There was no consistent relationship between the magnitude of the impact and the presence of an on-site day care center. For example, the lowest impact was in San Jose, which had an on-site day care center, although one rarely used by New Chance participants because slots were not reserved for them. By contrast, in four of the six sites *without* an on-site facility (Inglewood, Jacksonville, Minneapolis, and Philadelphia), the experimental/control group difference exceeded 24 percentage points. The most sizable impacts, however, were observed in two sites with on-site child care: Detroit (51 percentage points) and Salem (63 percentage points). Thus, on-site child care appears to have contributed to the observed impacts, but having on-site care does not completely account for the higher use of center care among the experimental group.

IV. Child Health

The children in this sample, all of whom lived in economically disadvantaged households, were expected to be at higher-than-average risk of health problems, given the evidence linking poverty to a broad range of poor health outcomes in children (Dawson, 1991; Hughes et al., 1989; Mott and Quinlan, 1991). According to program guidelines, New Chance programs were required either to provide health care to participants and their children directly on site or to create linkages with specific hospitals and clinics to which they could be referred. On-site health care was available in three sites, and many other sites established relationships with health care providers. In addition, child health issues were covered in both the health education and parenting components of the program. For example, classes sometimes covered such topics as infant nutrition, hygiene, and childhood immunizations.

This section describes health and health care outcomes for the focal child at 18 months and 42 months after random assignment. As was true for maternal health measures, the child health measures are limited and based entirely on maternal report rather than on objective physiological information or data from medical records.

Table 8.5 summarizes program impacts on the child health measures available in the two follow-up surveys. As this table indicates, there were few program impacts on child health outcomes collected in either of the two follow-up surveys.

In both surveys mothers were asked to characterize the focal child's health as "excellent, very good, good, fair, or poor." At both 18 and 42 months, about 80 percent of all respondents said that their child's health was either excellent or very good. There was no program effect on this outcome. On the other hand, some focal children (5.1 percent in the experimental group and 4.2 percent in the control group) were reported to have an ongoing physical or emotional condition that prevented the mother from working or going to school.[15]

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[15] All together, 8.4 percent of the mothers said that at least one of their children had such a health problem. The most commonly reported problems were asthma (46.4 percent of those with such a condition), hyperactivity (11.3 percent), heart trouble (6.5 percent), and epilepsy (6.0 percent).

Table 8.5

Impacts of New Chance on Child Health Outcomes for Focal Child at or Within 18 and 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Mother rated child's health as excellent or very good (%)				
at 18-month follow-up	78.5	78.8	-0.3	0.894
at 42-month follow-up	81.9	81.9	-0.1	0.967
Child has an ongoing physical or emotional condition that prevents mother from working or going to school (at 42-month follow-up) (%)	5.1	4.2	0.9	0.362
Average number of days child stayed in bed more than 1/2 day because of illness/injury				
Random assignment to 18-month follow-up	2.9	2.9	0.0	0.920
19-month to 42-month follow-up	3.6	3.6	0.1	0.905
Child hospitalized at least once (%)				
Random assignment to 18-month follow-up	13.7	13.5	0.2	0.920
19-month to 42-month follow-up	10.1	11.9	-1.8	0.229
Child had an injury, poisoning, or accident requiring medical attention (%)				
Random assignment to 18-month follow-up	23.0	24.4	-1.4	0.521
19-month to 42-month follow-up	23.1	19.7	3.4 *	0.089
Child experienced an emotional trauma in prior two years at 42-month follow-up (%)	43.4	40.8	2.6	0.262
Child had either Medicaid or private health insurance (%)				
at 18-month follow-up	92.5	90.5	2.0	0.126
at 42-month follow-up	95.3	93.7	1.6	0.116
Child had a period without any health insurance in past two years at 42-month follow-up (%)	14.5	14.4	0.1	0.951
Child saw a dentist within past 12 months at 42-month follow-up (%)	71.3	70.1	1.1	0.602
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

(continued)

Table 8.5 (continued)

[a]A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

The mothers were also asked to report the number of days during which the child had spent at least half the day in bed because of illness or injury in each of the two follow-up periods. The average number of illness-days for the two groups was virtually identical at both the 18-month and 42-month interviews. The number of illness-days at the final interview ranged from 0 to 180 and was severely skewed, with about 45 percent of the mothers reporting none and about 7 percent reporting more than 10 such days.

Despite the fairly positive picture of health suggested by the health ratings and despite the young age of the focal child, about one out of every eight children in both groups had been hospitalized (that is, been a patient in a hospital overnight or longer) at least once between baseline and the first follow-up interview, and just over 10 percent had been hospitalized in the two years between follow-ups. Group differences for this outcome were not statistically significant.

Between random assignment and the 18-month interview, nearly one-fourth of the children (23.0 percent of the experimentals and 24.4 percent of the controls) had had an injury, poisoning, or accident that sample members reported requiring medical attention. At the 42-month survey, 23.1 percent of the experimental group mothers and 19.7 percent of the control group mothers reported that the focal child had had such an injury between the two interviews. The latter finding was found to be statistically significant, although the difference was small. Also, the impact on the child's being hospitalized was actually negative (albeit statistically insignificant) and experimental group mothers may have sought medical attention for their children more often, either because of increased awareness due to their participation in New Chance or because they had somewhat greater access to health care, as discussed below.

In the 42-month interview, mothers were asked if anything had happened in the previous two years that particularly upset or disturbed the focal child. A somewhat higher percentage of focal children in the experimental group (43.4 percent) than in the control group (40.8 percent) had experienced such an emotional upset, but the difference was not statistically significant. There was no clear-cut difference in the type of upsetting experiences for the children in the two groups (not shown).

Mothers were also asked several questions about the focal child's health care. First, the surveys inquired about health insurance that covered the child's medical expenses. Despite the fact that most children were living in poverty, the overwhelming majority of children in both groups (over 90 percent) had health care insurance at both follow-up interviews, partly because many had access to Medicaid. At both follow-up interviews, experimentals were somewhat more likely to have health insurance for their children, although the differences were not statistically significant. Just under 15 percent of women in both groups, however, reported that the focal child had had a period without health care coverage in the two years prior to the final interview. Finally, the percentages of focal children in the two groups who had seen a dentist in the year before the 42-month survey were similar (about 70 percent). None of these group differences was statistically significant.

V. Child Development Outcomes

A large body of research evidence has documented that children raised in poor and welfare-dependent families fare worse than other children in their intellectual development, socioemotional development, and behavioral problems (Bolger et al., 1995; Geronimus, Korenman, and Hillemeier, 1994; Huston, McLoyd, and Garcia-Coll, 1994; McLoyd, 1990; Moore et al., 1994). Changes in welfare policies over the past 10 years have given rise to considerable concern about the possible effects that interventions for single welfare mothers might have on children (see, for example, Smith, 1995). On the one hand, programs that facilitate or promote employment are viewed as having the potential to help children as a result of eventual improvements to family income. On the other hand, there are concerns that such programs could increase maternal stress and could expose young children to inadequate child care. These concerns were not expected to be relevant in New Chance. As has been discussed, New Chance was expected to have positive effects on child development because of anticipated beneficial effects on a wide variety of maternal outcomes and because of the provision of high-quality child care.

A. Measures of Child Development

At the 18-month follow-up, the focal children were too young and too varied in age for developmental assessments to be made within the study's budgetary constraints. The 42-month follow-up interview, however, included a variety of child development measures: a scale administered directly to the focal child, assessments made by the mother, and — for children in school or in a Head Start program — assessments made by the child's teacher. This section provides a brief description of those measures.

1. **Bracken Basic Concept Scale**. The Bracken Basic Concept Scale (BBCS) is a measure of cognitive development that assesses a child's mastery of basic concepts. The full BBCS has 11 subtests, each covering a specific concept area. The first five subtests (colors, letter identification, numbers/counting, comparisons, and shapes) can be scored as a measure called the School Readiness Component (SRC). The SRC was the measure of cognitive development used in New Chance. The BBCS can be used with children age 2 years and 6 months to 7 years and 11 months, an age span that covered 95.9 percent of the New Chance focal children at the 42-month interview.[16]

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[16] The Bracken Basic Concept Scale was selected instead of the widely used Peabody Picture Vocabulary Test (PPVT) for several theoretical, methodological, and practical reasons. Like the PPVT, the BBCS is brief, easy to administer, and appropriate for a wide age range corresponding to the ages of the focal children. Also like the PPVT (with which BBCS scores have been found to correlate highly), the BBCS has shown strong evidence of reliability. The main reasons for selecting the BBCS over the PPVT were that (1) this test, which includes concepts intended to be indicators of school readiness, seemed a better measure with which to evaluate the New Chance intervention than the PPVT, which is a more generalized vocabulary test; (2) the BBCS was judged to be less culturally biased than the PPVT by a well-respected African-American child development specialist; and (3) the BBCS is easier to administer by lay interviewers and has a better floor for low-ability children than the PPVT.

In giving the BBCS, a test administrator states a concept and then shows the child a plate with 4 (or more) alternative pictures; the child must point to the picture corresponding to the concept. For the five SRC subscales, there are no basals to establish; every child begins at item 1. The ceiling for each subscale is established when the child incorrectly answers three consecutive items. The SRC requires approximately 10 to 15 minutes to administer.

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Internal consistency reliability in the standardization sample was high for both the SRC subtests (.90) and the total test (.97). Test-retest reliability for a two-week interval was also high (.98 for the SRC and .97 for the total BBCS). In the New Chance sample, the internal consistency reliability of the BBCS (SRC) was .98.[17]

2. **Behavior Problems Index.** The Behavior Problems Index (BPI) is a 26-item scale (a 28-item scale for school-age children) used to describe the incidence of behavioral problems of children age 4 or older. The BPI has been widely used by researchers and was administered in the Child Supplement portion of the NLSY. The BPI is completed by an adult knowledgeable about the child's behavior, usually a parent. In the New Chance 42-month follow-up, the BPI was completed by the mothers of all children in a self-administered format.[18]

The BPI has six subscales capturing different dimensions of a child's problematic behaviors. The six subscales are as follows, with a representative item for each:

- *Antisocial:* "My child bullies or is cruel or mean to others."
- *Anxious/Depressed:* "My child feels or complains that no one loves him or her."
- *Headstrong:* "My child argues too much."
- *Hyperactive:* "My child is impulsive, or acts without thinking."
- *Dependent:* "My child clings to adults."
- *Peer Conflict/Withdrawn:* "My child has trouble getting along with other children."

Raw BPI scores for the total scale and all subscales were converted to normed standard scores on the basis of national norms established in the 1981 National Health Interview Survey. The normed scores are based on single years of age data, and the scores are normed separately for boys and girls and for children who are in school and not yet in school. The national norms have a mean of 100 and a standard deviation of 15.

The internal consistency reliability of the BPI has generally been in the mid- to high .80s (for example, in the 1988 NLSY, the reliability for school-age children was .90). The reliability for

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Raw scores on the SRC can be converted to age-based standard scores (standardized in six-month intervals) that permit comparisons with the national sample used to standardize the test.

[17] More detailed information about the SRC as used in New Chance, including information on concurrent validity, is available in Polit, 1996a).

[18] The BPI was selected as a primary measure of the focal child's socioemotional development in New Chance because it is relatively brief, covers a broad developmental range, has an acceptable reading level, has been widely used, and had adequate reliability and validity in previous studies. A maternal-report instrument was selected because direct measures of the child, many of whom were quite young, were not possible within budgetary constraints.

the total maternal-report BPI in the New Chance sample was .82 for preschool children and .86 for school-age children. The reliability coefficients for the six subscales ranged from .49 to .63.

3. **Positive Behavior Scale**. A second measure of the focal child's socioemotional development was used to supplement the BPI, to avoid relying exclusively on an instrument that emphasized problem behaviors rather than more positive aspects of the child's development. The Positive Behavior Scale (PBS) was developed specifically for this study.[19] The PBS is a 25-item scale that was designed to capture constructs such as self-esteem, social competence, self-control, and obedience. Care was taken in developing the PBS to ensure that the reading levels were appropriate for the New Chance sample. The PBS was completed by all mothers in the New Chance 42-month sample in a self-administered format.

A factor analysis of the New Chance PBS data resulted in three stable and reliable dimensions. These three dimensions, which were used as the basis for creating subscales, are Compliance/Self-Control, Social Competence/Social Sensitivity, and Autonomy. A representative item from each subscale follows:

- *Compliance/Self-Control:* "My child is obedient, follows rules."
- *Social Competence/Social Sensitivity:* "My child shows concern for other people's feelings."
- *Autonomy:* "My child can easily find something to do on his or her own."

The internal consistency reliability coefficients for the PBS total scale was .94. Subscale reliabilities ranged from .77 to .88. Thus, the reliability for the newly created Positive Behavior Scale compares favorably with the reliability for the BPI. The scores on the maternal BPI and PBS were, as might be expected, strongly correlated, $r = -.54$.

4. **Teacher Questionnaire**. In addition to information about the focal child provided by the mother, data were obtained from the teachers of those children in the sample who were in school or in a Head Start program by the time of the 42-month survey.[20] A total of 844 teachers completed mailed questionnaires, for a response rate of 87.1 percent.

The 10-page, self-administered New Chance Teacher Questionnaire covered a variety of topics, including ratings of the child's academic performance and school adjustment, grade progression, special needs, disciplinary records, and behavioral characteristics. Most of the questions on the survey were taken or adapted from questions on the teacher questionnaire that was part of the 1981 National Survey of Children. Also included were modified versions of the BPI and PBS. The teacher BPI was identical to the BPI administered to mothers of school-age children in the 42-month survey, except that one item concerning the child's obedience at home was omitted. The teacher version of the PBS, also a 25-item scale, was identical to the mother's version except for the substitution of three items more appropriate in an educational context. The internal consistency reliabilities for the teacher versions of these scales was excellent: .93 for the total BPI (with subscales ranging from .62 to .85) and .97 for the total PBS (with subscales ranging from .89 to .95).

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[19] A review of available scales that measure positive child characteristics failed to reveal a scale that seemed suitable for use with an educationally and economically disadvantaged sample. As a major example, the 100-item Block and Block (1980) California Child Q Set (CCQS), which has been used and adapted by child development scholars for decades, has a number of items that are inappropriate for those with low reading skills (for example, "My child tries to manipulate others by ingratiation").

[20] More detailed information regarding the New Chance Teacher Questionnaire is available in Polit, 1996b.

B. Descriptive Overview

The findings suggest that the focal children in the New Chance sample are, as a group, at a substantial developmental disadvantage in comparison with other same-age children. On the Bracken Basic Concept Scale, for example, the children in the sample had a mean standard score of 6.9 — the equivalent of an 84 on a 100-point standard score scale — which translates to the 15th percentile nationally. Only 17 percent of the children in the sample scored at or above the national 50th percentile on the BBCS.

On the Behavior Problems Index completed by the mothers, the mean standard score (which represents the raw scores translated to nationally normed scores) was 109.2. In other words, in comparison with same-age and same-gender children nationally, the focal children in the New Chance sample scored 9.4 points (.63 standard deviation units) higher, indicating higher problem behaviors as perceived by their mothers. This finding is consistent with a study using NLSY data that found that children in welfare families had average BPI scores of about 107.5, even after controlling for maternal demographic characteristics and poverty (Moore & Driscoll, 1996).

Teachers reported fewer behavior problems than did mothers of school-age children — perhaps because the children behaved differently at home and in school, or perhaps because the mothers and the teachers had different perspectives on problem behaviors.[21] Unfortunately, the teachers' BPI raw scores cannot be translated to national norms, but the teachers also compared the focal children with other children in their classrooms. In general, the focal children were viewed as being about average in terms of school adjustment, self-esteem, motivation, and getting along with peers and teachers. With regard to academic indicators, teachers rated the verbal ability, mathematical ability, and overall academic performance of these children at about the middle of their classes.

C. Aggregate Program Impacts on Child Development Outcomes

Since New Chance was designed as a two-generational program, it was hoped that there would be positive program impacts on the participants' children. New Chance does not, however, appear to have improved the developmental outcomes for sample members' children.

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[21] The correlation between mothers' and teachers' BPI scores was .30. The correlation between the mothers' and teachers PBS scores was also .30.

Table 8.6 presents impact results for several child development outcomes. As this table shows, the scores on the School Readiness Component of the Bracken Basic Concept Scale for experimental group children (mean standard score = 6.6) and control group children (mean standard score = 6.9) were not statistically significantly different. In both groups, the average child was at about the 15th percentile nationally.

As measured by the mothers' reports on the BPI scale, experimental group children had somewhat more behavioral problems than children in the control group (means of 110.0 and 108.5 for experimentals and controls on the normed BPI scale, respectively — a statistically significant difference of a tenth of a standard deviation). Children whose mothers were in the experimental group had higher average scores on all six subscales of the BPI, with differences reaching statistical significance on three of them — the Anxious/Depressed, Hyperactive, and Peer Conflict/Withdrawn subscales. Mothers in the experimental group also rated the focal child less favorably on the Positive Behavior Scale than did mothers in the control group (mean = 192.1 for experimentals, 197.3 for controls). In absolute size this negative program effect of 5.3 points is small, but it is statistically significant. The experimental/control difference in the overall PBS score was reflected in each of the three subscales of the PBS. Thus, control group mothers rated their children more favorably than did experimental group mothers in terms of their compliance, social competence, and autonomy.

The picture is somewhat different when teacher reports are considered. Table 8.7 summarizes program impacts on child development outcomes for those children in the New Chance sample for whom teacher survey data were available. With respect to the teacher-reported Behavior Problems Index, Table 8.7 shows the total scores as raw scores — that is, the actual number of behavior problems attributed to the focal child from the 27 items on the teacher's version of the BPI. The teachers attributed a similar number of problem behaviors to the children of experimentals (mean = 7.9) than to the children of controls (mean = 8.4); this difference was not statistically significant. Similarly, there were no experimental/control differences on any of the BPI subscales as reported by teachers (not shown). Note that Table 8.7 also presents maternal-report BPI raw scores for those children for whom there is teacher survey data — that is, for the same children for whom there is a teacher version of the BPI. For the subset of children in the teacher survey, mothers in the experimental group — unlike the teachers — attributed significantly more behavior problems to the focal children than those in the control group. Note that mothers in *both* groups attributed more problem behaviors to their children than the teachers did.[22] However, the discrepancy between the mothers' and the teachers' perceptions was greater for those in the experimental group (average

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[22] To permit a meaningful comparison between maternal and teacher reports of the focal child's problem behaviors, the BPI means for the mothers as shown in Table 8.7 were scored with the item on disobedience at home (the item not on the teacher BPI) removed. The standardized maternal BPI scores are also shown in Table 8.7 to facilitate a comparison of this subset of children with those in the overall sample (Table 8.6). It appears that children in the teacher survey subsample were somewhat better behaved that those who were not; for example, the mean BPI score for controls in the subsample was 107.6, compared with 108.5 for controls overall. The small difference is not attributable entirely to selection bias (that is, teacher's nonresponse or mother's refusal to allow researchers to contact the teacher) but also reflects the fact that younger children in the sample generally were not eligible for the teacher survey because they were not yet in school.

Table 8.6

Impacts of New Chance on Child Development Outcomes for Focal Child at 42 Months After Random Assignment

Outcome	Experimentals	Controls	Difference	p[a]
Standard score on School Readiness Component of Bracken Basic Concept Scale (BBCS),[b] at 42-month follow-up	6.6	6.9	-0.2	0.103
Behavior Problems Index (BPI),[c] at 42-month follow-up				
Total BPI scale, standard score	110.0	108.5	1.5 **	0.026
BPI subscale standard scores				
Antisocial	110.3	110.0	0.4	0.585
Anxious/Depressed	107.4	105.7	1.6 ***	0.005
Headstrong	102.7	102.6	0.1	0.827
Hyperactive	109.4	107.8	1.6 **	0.019
Dependent	109.1	108.2	0.8	0.182
Peer Conflict/Withdrawn	108.0	105.9	2.1 ***	0.002
Positive Behavior Scale (PBS),[d] at 42-month follow-up				
Total PBS scale score	192.1	197.3	-5.3 ***	0.001
PBS subscale scores				
Compliance	60.8	63.2	-2.4 ***	0.002
Social Competence	88.9	90.7	-1.8 **	0.014
Autonomy	42.4	43.4	-1.0 ***	0.002
Sample size	1,401	678		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Bracken Basic Concept Scale (BBCS) is a measure of receptive language that assesses the mastery of basic concepts; the School Readiness Component consists of five subtests of the BBCS: colors, letter identification, numbers, comparisons, and shapes. The scores shown are standard scores on a scale that ranges from 1 to 19; a standard score of 6.9 corresponds to about the 15th percentile nationally.

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Table 8.6 (continued)

[c]The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[d]The Positive Behavior Scale (PBS) is a 25-item scale developed for this study, with many items adapted from the Block and Block California Child Q Set. Scores for the total scale could range from zero (least favorable score) to 250 (most favorable score). The subscales were developed on the basis of a factor analysis.

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Table 8.7

Impacts of New Chance on Child Development Outcomes at 42 Months After Random Assignment, for Children in the Teacher Questionnaire Subsample

Outcome	Experimentals	Controls	Difference	p[a]
Behavior Problems Index,[b] at 42-month follow-up				
Raw total score, teacher survey	7.9	8.4	-0.4	0.389
Raw total score, maternal report[c]	10.1	9.2	0.9 **	0.028
Standardized total score, maternal report	109.6	107.6	2.1 **	0.043
Positive Behavior Scale,[d] at 42-month follow-up				
Raw total score, teacher survey	166.7	167.2	-0.4	0.909
Raw total score, maternal report	195.0	200.9	-5.9 **	0.018
Average teacher rating (1-5) of child's academic performance/ability				
Verbal ability	2.9	2.8	0.1	0.507
Mathematical ability	3.2	3.1	0.1	0.217
Overall academic performance	3.2	3.0	0.1	0.228
Average maternal rating (0-10) of child's academic performance	8.2	8.5	-0.3 *	0.079
Teacher rated child as worse than other students in class in terms of: (%)				
Getting along with other students	15.7	19.3	-3.6	0.197
Getting along with teachers	11.8	11.6	0.2	0.942
Self-esteem	22.0	23.7	-1.7	0.582
Motivation to do well	26.2	25.7	0.6	0.862
Overall school adjustment	17.6	19.5	-1.9	0.516
Teacher's report of disciplinary problems (%)				
Has taken disciplinary action more than once a week	30.2	27.1	3.1	0.358
Has had a behavior problem requiring parental notification	26.7	28.7	-1.9	0.545
Has had multiple behavior problems requiring parental notification	15.9	12.5	3.4	0.182
Maternal report of school notification of behavior problem (%)	23.0	17.3	5.7 *	0.089
Teacher reported meeting with mother for a general discussion of child (%)	82.4	83.4	-1.0	0.734
Sample size	554	281		

SOURCES: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 835 children for whom a teacher questionnaire was completed and there were 42-month follow-up survey data on selected child development outcomes. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some questionnaires.

(continued)

Table 8.7 (continued)

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[c] For the purposes of this table, the maternal BPI was scored with the item on disobedience at home (the item not on the teacher BPI) removed. Thus, the raw scores of mothers and teachers can be directly compared.

[d] The Positive Behavior Scale (PBS) is a 25-item scale developed for this study, with many items adapted from the Block and Block California Child Q Set. Scores for the total scale can range from zero (least favorable score) to 250 (most favorable score). The subscales were developed on the basis of a factor analysis.

discrepancy of 2.2 problems) than for those in the control group (average discrepancy of .8 problems).

On the Positive Behavior Scale, the teachers gave comparably favorable ratings of positive behaviors to children in both groups, both on the total PBS (Table 8.7) and on all three subscales (not shown). However, among the mothers of children in the teacher survey subsample, the mean ratings on the total PBS were higher (that is, more favorable) for controls (mean = 200.9) than for experimentals (mean = 195.0). Although mothers in both research groups attributed more behavior problems to their children than did teachers, mothers in both groups also rated their children's positive qualities more highly than did teachers.

Teachers were asked to rate the focal children with respect to their verbal ability, mathematical ability, and overall academic performance, on a scale from 1 ("one of the best in the class") to 5 ("near the bottom of the class"). The average ratings on all three indicators was near the midpoint of 3 ("in the middle of the class") for both research groups; none of the group differences was statistically significant. When mothers, however, were asked to rate how their children were doing in school on a scale from 0 ("very poorly") to 10 ("extremely well"), those in the experimental group gave significantly lower ratings (mean of 8.2) than those in the control group (mean of 8.5).[23] It is not clear whose reports are more accurate. While teachers are better able to evaluate a child's school performance in comparison with other children, many teachers based their ratings on only several months of contact with the child. Mothers, on the other hand, would have based their ratings on information from several teachers if their children were old enough to be in school or in a structured program like Head Start for more than one year.

Teachers were also asked to rate the children in terms of such characteristics as getting along with other students and overall school adjustment, on a scale from "much better than other students" to "much worse than other students." Table 8.7 shows the percentage of children in each group rated as "worse" or "much worse" than other students on the various traits. A minority of children in the two groups were judged to be worse than their peers in terms of getting along with teachers or other students, self-esteem, motivation, and overall school adjustment. The two groups were given comparable ratings by the teachers on all five traits.

Various questions on the teacher questionnaire concerned disciplinary actions involving the focal child. Teachers reported at least one disciplinary action each week for almost 30 percent of the children (30.2 percent of the experimental group children and 27.1 percent of the control group children); the group difference was not significant. For 15.9 percent of the children in the experimental group and 12.5 percent of children in the control group, teachers reported a classroom behavior problem that resulted in "the student's parents being sent a note or being asked to come in to meet with [the teacher] or the principal" on more than one occasion. The experimental/control difference was not statistically significant ($p = 0.182$). Significantly more mothers in the experimental group (23.0 percent) than in the control group (17.3 percent) acknowledged that they had been notified of a disciplinary problem at their child's school.

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[23] Because of space constraints, Table 8.7 does not include teacher reports on a number of other outcomes, such as repeating a grade, school absences, school tardiness, teacher-parent interactions, and needs for special services. Group differences on these outcomes generally were not statistically significant. The one exception is that teachers were significantly more likely to say that experimental children (1.3 percent) needed special education services for mental retardation than that control children needed them (0.0 percent, p = .018).

Finally, teachers were asked several questions regarding contact with the focal child's parents. According to the teachers, experimental group mothers were as likely as control group mothers to have met with the focal child's teacher for a discussion of the child (82.4 percent and 83.4 percent, respectively).

Overall, then, the findings suggest that the program did not have the hoped-for positive effects on the children's cognitive or socioemotional development. It is even possible that the program had unintended negative effects. If so, the negative program effects on the mothers' BPI and PBS scales might be influenced by the mother's emotional state at the time of the 42-month interview. Mothers in the experimental group, it will be recalled, were significantly more depressed and more stressed as parents than those in the control group, and this fact may have affected their perceptions of the child's behavior and school performance.

D. Subgroup and Site Impacts on Child Development Outcomes

Although there were no program effects on the standardized Bracken Basic Concepts Scale scores for the aggregate sample, there were a few statistically significant impacts for subgroups of the New Chance sample (see Table 8.8). Among children who were younger than 18 months at baseline and children of Hispanic mothers, of mothers reading at the 10th-grade level or higher at baseline, of mothers whose families were never on welfare as a child, and of mothers at high risk of depression, children in the control group had significantly higher BBCS scores than children in the experimental group. The difference in impacts across the subgroup defined on the basis of depression scores at random assignment was significant. At the site level, experimental children in the Bronx and Chicago scored significantly lower on the BBCS than their control group counterparts, but the between-site impacts test was not statistically significant (not shown).

With respect to subgroup impacts on the maternal BPI scores, Table 8.9 shows that children of experimental group members had higher scores than children of control group members in almost every subgroup, and in many cases the impact was statistically significant. Such impacts on BPI scores were found for boys but not for girls, and for older but not younger children. Group differences favoring controls were especially large for the children of women at high risk of depression at baseline, women who had been out of school for more than two years at baseline, and women who were older than 19 when they applied to the program. The unfavorable impact on the BPI was greatest for sample members who scored high on the multiple risk score. The difference in impacts was almost statistically significant across the three depression subgroups and was statistically significant across the subgroups divided on the basis of baseline age, length of time out

Table 8.8

Impacts of New Chance on Focal Child's Bracken Basic Concept Scale Standard Scores at 42 Months After Random Assignment, for Selected Subgroups

Maternal Characteristic and Subgroup at Random Assignment	Sample Size	BBCS Standard Score		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						---	0.757
16-17	402	6.2	6.6	-0.4	0.233		
18-19	997	6.7	6.8	-0.1	0.528		
20-22	678	6.9	7.2	0.3	0.277		
Ethnicity						---	0.342
Black, non-Hispanic	1,087	6.5	6.6	-0.1	0.680		
Hispanic	474	6.4	7.0	-0.6 **	0.046		
White or other	515	7.3	7.5	0.2	0.426		
Highest grade completed						0.3	0.357
10th or below	1,391	6.5	6.7	-0.1	0.410		
11th or above	684	7.0	7.4	-0.4 *	0.088		
Interval since last attended regular high school						-0.2	0.501
More than 2 years	1,093	6.6	6.9	-0.3	0.110		
2 years or less	927	6.7	6.8	-0.1	0.553		
TABE reading test score (grade equivalent)[c]						---	0.403
Below 6th grade	433	6.2	6.4	-0.3	0.375		
6th or 7th grade	492	6.6	6.6	-0.2	0.934		
8th or 9th grade	566	6.8	6.8	-0.2	0.928		
10th grade or above	583	7.0	7.6	-0.6 **	0.028		
Family received AFDC when sample member was growing up						---	0.115
Always	341	6.5	6.4	0.1	0.769		
Sometimes	970	6.6	6.7	-0.1	0.821		
Never	749	6.7	7.4	-0.6 ***	0.010		
Ever employed						-0.1	0.729
Yes	1,646	6.7	6.9	-0.3	0.110		
No	433	6.6	6.8	-0.1	0.663		
CES-D (depression) Scale[d]						--- *	0.081
0-15 (not at risk)	967	6.9	6.8	0.1	0.598		
16-23 (at some risk)	525	6.5	7.0	-0.5 *	0.077		
24-60 (at high risk)	582	6.4	7.0	0.6 **	0.040		
Multiple risk score[e]						---	0.755
Low	871	6.8	7.0	-0.2	0.405		
Moderate	618	6.6	7.0	-0.3	0.200		
High	525	6.5	6.6	0.0	0.887		

(continued)

Table 8.8 (continued)

Maternal Characteristic and Subgroup at Random Assignment	Sample Size	BBCS Standard Score Experimentals	BBCS Standard Score Controls	Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
Focal child's gender						-0.2	0.514
Girl	952	6.9	7.2	-0.3	0.122		
Boy	1,037	6.4	6.6	-0.1	0.511		
Focal child's age at baseline						-0.2	0.535
Less than 18 months	1,172	6.6	6.9	-0.3 *	0.097		
18 months or older	902	6.8	7.0	-0.1	0.550		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

The Bracken Basic Concept Scale (BBCS) is a measure of receptive language that assesses the mastery of basic concepts; the School Readiness Component consists of five subtests of the BBCS: colors, letter identification, numbers, comparisons, and shapes. The scores shown are standard scores on a scale that ranges from 1 to 19; a standard score of 6.9 corresponds to about the 15th percentile nationally.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

Table 8.9

Impacts of New Chance on Focal Child's Behavior Problems Index Standardized Scores at 42 Months After Random Assignment, for Selected Subgroups

Maternal Characteristic and Subgroup at Random Assignment	Sample Size	BPI Standardized Score		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Age (years)						--- *	0.068
16-17	402	109.7	111.2	-1.5	0.324		
18-19	997	109.4	107.8	1.6 *	0.091		
20-22	678	110.9	107.9	3.0 **	0.012		
Ethnicity						---	0.637
Black, non-Hispanic	1,087	109.3	107.6	1.8 **	0.048		
Hispanic	474	109.3	108.9	0.3	0.814		
White or other	515	112.0	110.0	2.0	0.165		
Highest grade completed						-0.9	0.515
10th or below	1,391	109.8	108.6	1.2	0.152		
11th or above	684	110.2	108.1	2.1 *	0.071		
Interval since last attended regular high school						3.1 **	0.023
More than 2 years	1,093	110.8	107.7	3.1 ***	0.001		
2 years or less	927	109.1	109.1	0.0	0.996		
TABE reading test score (grade equivalent)[c]						---	0.530
Below 6th grade	433	109.6	108.1	1.5	0.292		
6th or 7th grade	492	111.5	108.3	3.1 **	0.022		
8th or 9th grade	566	108.9	108.3	0.5	0.665		
10th grade or above	583	109.9	109.0	1.0	0.443		
Family received AFDC when sample member was growing up						---	0.619
Always	341	110.2	109.6	0.6	0.735		
Sometimes	970	110.3	108.1	2.2 **	0.022		
Never	749	109.4	108.3	1.1	0.342		
Ever employed						-2.1	0.196
Yes	1,646	110.2	108.3	1.9 **	0.011		
No	433	108.9	109.1	-0.2	0.893		
CES-D (depression) Scale[d]						---	0.112
0-15 (not at risk)	967	107.5	107.3	0.2	0.843		
16-23 (at some risk)	525	110.6	109.0	1.5	0.240		
24-60 (at high risk)	582	113.4	109.9	3.5 ***	0.005		
Multiple risk score[e]						--- **	0.013
Low	871	108.3	108.6	-0.3	0.773		
Moderate	618	110.1	108.5	1.6	0.178		
High	525	112.4	107.7	4.7 ***	0.001		

(continued)

Table 8.9 (continued)

Characteristic and Subgroup at Random Assignment	Sample Size	BPI Standardized Score		Within-Subgroup Impact	p[a]	Difference Across Subgroup Impacts[b]	p[a]
		Experimentals	Controls				
Focal child's gender						-1.4	0.276
Girl	952	109.3	108.6	0.7	0.452		
Boy	1,037	110.5	108.3	2.2 **	0.020		
Focal child's age at baseline						-0.8	0.568
Less than 18 months	1,172	110.7	109.6	1.1	0.212		
18 months or older	902	108.9	107.1	1.9 *	0.062		

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages are adjusted using a two-way analysis of covariance procedure controlling for up to 51 kinds of difference in characteristics, other than the characteristic used to define subgroups, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and, one at a time, the baseline characteristics indicated. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each within-subgroup impact. The statistical significance of differences in impacts across subgroups was tested with a t-test (when there were two subgroups) or an F-test (when there were more than two). The columns labeled "p" indicate the level of significance associated with each test: That is, p is the probability that the estimated impacts are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] For each characteristic with only two subgroups, the difference across subgroup impacts is the impact for the first subgroup less the impact for the second subgroup. For characteristics with more than two subgroups, no single difference between subgroup impacts can be calculated, as indicated by dashes in the table. However, it is possible to assess the statistical significance of variation across multiple subgroups, as indicated by the asterisks.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[e] To capture the cumulative effect of multiple risks for unfavorable outcomes, five baseline characteristics were used: (1) being at high risk of depression (having a CES-D score of 24-60), (2) being out of school more than two years, (3) having more than one child, (4) having been on welfare continuously as a child, and (5) having a reading level below eighth grade. Low risk was defined as having none or one of these characteristics; moderate risk, two characteristics; and high risk, three or more.

of school, and multiple risk score.[24] Among the sites, there was a statistically significant impact on BPI scores from maternal reports, all favoring controls, in three sites — Allentown, the Bronx, and Lexington (not shown). The difference in impacts across the sites was not statistically significant (p = .21).

These subgroup analyses suggest that the program's impacts on child development outcomes varied by the focal child's gender and age. Older children and male children seemed especially susceptible to negative impacts on socioemotional outcomes. Further analyses examined program impacts when the child's gender and age were considered simultaneously. Table 8.10 presents impacts on selected child development outcomes for focal children who were younger than 18 months at baseline; impacts are shown separately for boys and girls. The outcomes shown in this table include the Bracken Basic Concept Scale, the two child behavior scales (BPI and PBS) as reported by mothers, and — for confirmatory evidence — some interviewer ratings of the child's behavior, based on observations during the 42-month interview, which was, on average, about 90 minutes long.

Table 8.10 indicates that there were no program impacts on younger girls in the sample. Differences between the two research groups for the younger girls sometimes favored controls and sometimes favored experimentals, but in all cases they were small and rarely even approached statistical significance. By contrast, there were several program impacts on younger boys (who had less favorable outcomes than the younger girls, in both groups). Mothers in the experimental group attributed more behavior problems (especially problems that are indicative of high anxiety) and less positive behavior to their young boys than did the mothers in the control group.[25] The absolute magnitude of these differences was small, but they were statistically significant. The group difference on Bracken scores for the boys also favored controls and narrowly missed statistical significance, p = .107. Maternal perceptions of these younger boys' behavior had some corroboration from the interviewers. According to the interviewers, significantly more boys in the experimental group (7.3 percent) than in the control group (3.0 percent) had a tantrum while the interviewer was in the home. Moreover, interviewers reported that significantly more boys in the experimental group (27.9 percent) than in the control group (20.7 percent) failed to make spontaneous attempts to get their mothers' attention through positive behaviors (for example, waving hello, smiling at the mother, showing her something) during the interview session. These

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[24] The subgroup results for the Positive Behavior Scale (not shown) were similar to results for the BPI. In every subgroup considered, the control group mothers rated their children more favorably than experimental group mothers, and the difference was significant for a large number of subgroups. The impact was especially large for those at high risk of depression (an 11.0 point difference on the PBS scale), and the difference in impacts across depression subgroups was significant.

[25] In this subgroup of younger male focal children, mothers in the experimental group also had significantly higher scores on the Parenting Stress Scale (mean = 27.9) than mothers in the control group (mean = 25.0, p = .04); differences on the Aggravation subscale were even more substantial. Moreover, there was also an impact on the Harsh Discipline subscale of the HOME for the younger boys; the mean in the experimental group (96.9) was significantly lower than the mean for controls (100.0, p = .035), indicating more use of harsh punishment among experimental mothers (not shown). It is noteworthy that two of the five items on the Harsh Discipline subscale of the HOME are based on interviewer observations of harsh discipline, rather than on maternal reports.

Table 8.10

Impacts of New Chance on Selected 42-Month Child Development Outcomes, for Focal Children Who Were Younger Than 18 Months at Random Assignment

	Boys				Girls			
Outcome	Experimentals	Controls	Difference	p[a]	Experimentals	Controls	Difference	p[a]
Bracken Basic Concepts Scale[b] standard score	6.3	6.7	-0.4	0.107	6.9	7.1	-0.1	0.592
Maternal Behavior Problems Index[c] raw scores								
Total scale	11.5	10.7	0.9 *	0.088	10.2	10.2	0.1	0.902
Subscale								
Antisocial	1.9	1.8	0.1	0.246	1.6	1.7	-0.1	0.258
Anxious/Depressed	1.8	1.5	0.3 ***	0.005	1.6	1.6	0.0	0.978
Headstrong	2.7	2.6	0.2	0.281	2.5	2.5	0.0	0.946
Hyperactive	2.4	2.3	0.1	0.349	2.1	2.0	0.1	0.588
Dependent	2.1	2.0	0.0	0.689	2.0	2.0	0.0	0.670
Peer conflict/Withdrawn	0.6	0.5	0.1	0.400	0.5	0.4	0.1	0.311
Maternal Positive Behavior Scale[d] raw scores								
Total scale	185.0	192.7	-7.7 **	0.016	194.5	194.3	0.2	0.939
Subscale								
Compliance	56.6	60.4	-3.8 **	0.013	61.4	61.2	0.2	0.889
Social competence	86.9	89.5	-2.7 *	0.056	90.3	90.2	0.0	0.981
Autonomy	41.5	42.8	-1.3 *	0.077	42.9	42.9	0.0	0.993
Interviewer observations (%)								
Never disobeyed mother	47.0	43.8	3.2	0.496	55.4	53.1	2.3	0.649
Whined/cried at least once	19.3	20.5	-1.2	0.745	12.8	17.2	-4.4	0.213
Had a tantrum	7.3	3.0	4.3 *	0.068	2.7	4.4	-1.6	0.357
Never made positive attempts to get mother's attention	27.9	20.7	7.2 *	0.078	23.7	27.7	-4.0	0.326
Never sought positive physical contact with mother	60.2	60.3	-0.2	0.973	44.1	50.9	-6.8	0.147
Sample size	409	181			353	178		

(continued)

Table 8.10 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

Calculations for this table used data for 1,121 children who were younger than 18 months at random assignment, for whom there were 42-month follow-up survey data on selected child development outcomes. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of difference in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Bracken Basic Concept Scale (BBCS) is a measure of receptive language that assesses the mastery of basic concepts; the School Readiness Component consists of five subtests of the BBCS: colors, letter identification, numbers, comparisons, and shapes. The scores shown are standard scores on a scale that ranges from 1 to 19; a standard score of 6.9 corresponds to about the 15th percentile nationally.

[c] The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[d] The Positive Behavior Scale (PBS) is a 25-item scale developed for this study, with many items adapted from the Block and Block California Child Q Set. Scores for the total scale can range from zero (least favorable score) to 250 (most favorable score). The subscales were developed on the basis of a factor analysis.

findings suggest that there may have been some tensions in the mother-child relationship that were affected by New Chance.

Table 8.11 presents similar information for older children in the sample — that is, those who at baseline were 18 months or older and who at the final interview were older than five. This table includes only children for whom there was a teacher questionnaire, so that mother and teacher perspectives could be compared. For the older girls in the New Chance sample, there is considerable consistency across observers; mothers, teachers, and interviewers all attributed more negative behaviors to experimental girls than to control girls. For example, older girls in the experimental group were described by their mothers as having significantly more behavior problems (mean = 10.1) than those in the control group (mean = 8.5).[26] Although there were no significant group differences on the teacher version of the BPI, teachers did report that significantly more of the girls in the experimental group (9.5 percent) than in the control group (2.9 percent) had recurring behavior problems in school that required notification of the parent. Moreover, teachers rated girls in the experimental group significantly less favorably than girls in the control group on the Positive Behavior Scale. And during the 42-month interviews, significantly more girls in the control group (77.9 percent) than in the experimental group (67.2 percent) were described by interviewers as not having disobeyed their mothers. In addition to these program impacts on the older girls' behavior, there was also an impact on the Bracken test scores; those in the experimental group (mean = 7.0) scored significantly lower than those in the control group (mean = 7.6). Finally (and consistent with results for the BBCS), mothers in the experimental group rated their daughters' academic performance less positively than control group mothers (8.3 versus 8.8, respectively).

With regard to outcomes for the older boys in the New Chance sample, the findings in Table 8.11 are somewhat anomalous. For these children, there were only a few program effects on outcomes based on maternal reports, and those favored controls. For example, experimental group mothers rated their older boys as being more anxious and depressed than did control group mothers, and as being less autonomous. Impacts on outcomes based on teacher reports, however, appeared to favor the *experimentals*. For example, older boys in the control group were described by their teachers as having significantly more behavior problems (mean = 11.9) than their experimental group counterparts (mean = 9.9). The most substantial positive impact was on the Anxious/Depressed subscale of the teacher BPI — the very subscale for which a negative program impact was observed on the maternal BPI. Teachers also gave experimental boys higher ratings on the PBS. There were no statistically significant program effects on any of the interviewer observation measures for this subgroup.

This anomalous pattern of impacts for the older boys may be a result of the limited size of the sample and the diversity in teachers' perceptions. It is also possible, however, that the boys behaved differently in school and at home (for example, that behavior suggestive of anxiety and dependence emerged only within the context of mother-child interactions). Moreover, certain behaviors of these older experimental boys may have been evaluated more positively by teachers than by mothers. In any event, the findings in Table 8.11 suggest that the lack of consistency in the

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[26] Mothers of older girls in the experimental group also had significantly higher scores on the Parenting Stress Scale (mean = 25.1) than those in the control group (mean = 21.8, p = .064).

Table 8.11

Impacts of New Chance on Selected 42-Month Child Development Outcomes, for Children Who Were Older Than 18 Months at Random Assignment (Teacher Questionnaire Subsample)

	Boys				Girls			
Outcome	Experimentals	Controls	Difference	p[a]	Experimentals	Controls	Difference	p[a]
Bracken Basic Concepts Scale[b] standard score	6.9	6.6	0.3	0.417	7.0	7.6	-0.7 *	0.077
Average maternal rating (0-10) of child's academic performance	7.9	8.1	-0.2	0.522	8.3	8.8	-0.5 **	0.044
Maternal Behavior Problems Index[c] raw scores								
Total scale	11.7	10.8	0.9	0.236	10.1	8.5	1.6 **	0.041
Subscale								
Antisocial	2.2	1.9	0.2	0.326	1.6	1.3	0.3	0.146
Anxious/Depressed	2.0	1.6	0.3 **	0.038	1.8	1.6	0.2	0.145
Headstrong	2.6	2.5	0.1	0.805	2.0	2.0	0.0	0.962
Hyperactive	2.7	2.4	0.3	0.188	2.2	1.6	0.6 ***	0.005
Dependent	1.7	1.7	0.0	0.839	1.7	1.5	0.2	0.207
Peer conflict/Withdrawn	0.7	0.7	0.0	0.985	0.7	0.4	0.3 **	0.011
Teacher Behavior Problems Index[c] raw scores								
Total scale	9.9	11.9	-2.0 **	0.047	6.5	6.2	0.3	0.770
Subscale								
Antisocial	2.1	2.6	-0.5 *	0.075	1.2	1.2	0.1	0.828
Anxious/Depressed	1.6	2.2	-0.6 ***	0.007	1.2	1.2	0.0	0.876
Headstrong	1.5	1.8	-0.3	0.163	1.0	0.9	0.1	0.504
Hyperactive	2.7	2.9	-0.2	0.343	1.6	1.4	0.2	0.395
Dependent	1.0	1.2	-0.2	0.190	0.8	0.8	0.0	0.963
Peer conflict/Withdrawn	1.0	1.2	-0.2	0.206	0.6	0.7	-0.1	0.593
Maternal Positive Behavior Scale[d] raw scores								
Total scale	187.0	194.3	-7.3	0.118	203.2	207.1	-3.9	0.366
Subscale								
Compliance	59.5	62.7	-3.2	0.146	66.9	68.5	-1.6	0.437
Social competence	85.9	88.3	-2.4	0.212	92.7	93.8	-1.1	0.551
Autonomy	41.6	43.2	-1.6 *	0.097	43.6	44.8	-1.2	0.641

(continued)

Table 8.11 (continued)

	Boys				Girls			
Outcome	Experimentals	Controls	Difference	p[a]	Experimentals	Controls	Difference	p[a]
Teacher Positive Behavior Scale[d] raw scores								
Total scale	150.5	138.8	11.7 *	0.097	174.8	186.8	-12.1 *	0.061
Subscale								
Compliance	49.1	46.2	2.9	0.314	60.4	66.0	-5.6 **	0.042
Social competence	69.8	63.0	6.9 **	0.037	80.0	84.1	-4.1	0.151
Autonomy	31.6	29.7	1.9	0.201	34.3	36.7	-2.4 *	0.082
Teacher reports that child has had multiple behavior problems requiring parental notification (%)	27.4	29.5	-2.1	0.720	9.5	2.9	6.6 *	0.052
Interviewer observations (%)								
Never disobeyed mother	60.9	66.1	-5.2	0.406	67.2	77.9	-10.7 *	0.085
Whined/cried at least once	4.9	4.9	0.0	0.987	3.9	0.4	3.5	0.111
Had a tantrum	2.9	1.1	1.8	0.395	0.6	0.9	-0.3	0.773
Never made positive attempts to get mother's attention	41.2	41.5	-0.4	0.951	41.5	45.5	-4.0	0.512
Never sought positive physical contact with mother	76.5	84.8	-8.3	0.104	75.1	78.2	-3.0	0.585
Sample size	206	97			190	104		

SOURCE: MDRC calculations from New Chance survey data.

NOTES: Calculations for this table used data for 597 children older than 18 months at random assignment for whom a teacher questionnaire was completed and for whom there were 42-month follow-up survey data on selected child development outcomes. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some questionnaires.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The Bracken Basic Concept Scale (BBCS) is a measure of receptive language that assesses the mastery of basic concepts; the School Readiness Component consists of five subtests of the BBCS: colors, letter identification, numbers, comparisons, and shapes. The scores shown are standard scores on a scale that ranges from 1 to 19; a standard score of 6.9 corresponds to about the 15th percentile nationally.

(continued)

Table 8.11 (continued)

[c] The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[d] The Positive Behavior Scale (PBS) is a 25-item scale developed for this study, with many items adapted from the Block and Block California Child Q Set. Scores for the total scale can range from zero (least favorable score) to 250 (most favorable score). The subscales were developed on the basis of a factor analysis.

aggregate impacts between mothers and teachers (Table 8.7) is largely accounted for by the fact that opposing impacts on teacher ratings for boys and girls canceled each other out.

E. Perspectives on the Impacts on Child Development Outcomes.

The impact findings discussed in this section suggest that New Chance had some adverse effects on child development outcomes, especially for younger boys and older girls. Subgroup analyses showed these impacts to be concentrated among sample members who were found to be at high risk at baseline, and specifically among those at high risk of depression. In these subgroups, children in the experimental group were reported by their mothers to have less desirable behavior than control group children. Even though there was some evidence from teachers and interviewers substantiating the maternal reports, other findings were inconsistent, suggesting that at least part of this effect was attributable to the mothers' perceptions, rather than the children's behavior.[27] On the other hand, among younger boys and older girls, the children in the experimental group themselves performed more poorly than controls on the school readiness subtest of the Bracken Basic Concepts Scale. There was little evidence that the program intervention had any of the intended beneficial effects on children's development.

The findings are of considerable concern, given the intent of the program to have beneficial effects on both mothers and children. Additional analyses were undertaken to explore possible pathways of the program's negative effects on child development outcomes. The literature on child development suggests two possible explanations for the observed effects — both of which are consistent with the pattern of program impacts just described.

1. Child Care Effects on Children's Behavior. Despite evidence that good child care can have beneficial effects on low-income children, some research suggests that formal child care may have negative effects on children and on mother-child relationships. This is the case especially for children placed in care for a considerable number of hours per week before their first birthday, for boys, in cases where the child care arrangements are unstable, and in cases where mother and child relationships are weak to begin with. Attachment theory is often used to explain this body of findings. According to this theory, young children and their mothers are at increased risk of developing insecure attachments if a regular child care arrangement is used for more than 20 hours a week during the child's first year. Insecure attachment is associated with a number of child behavior problems (see Belsky, 1990, for a review). Since New Chance had powerful (albeit relatively brief) effects on the use of child care, it is possible that the negative program impact on children's behavior was mediated by the greater use of day care centers among experimentals compared with controls.

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[27] A separate analysis was conducted that compared the BPI score reported by the mother with the BPI score reported by the teacher for children for whom both of these outcomes were available. It was found that New Chance increased the discrepancy between these two scores, but *only* for mothers who were at risk of depression when they entered New Chance. For them, the program caused maternal scores to be substantially higher (more negative) than one would expect on the basis of the teacher's rating of the child's behavior.

To test whether this explanation has merit, an instrumental variables (two-stage least squares) regression analysis was performed.[28] In this analysis, the outcome used was the standardized total BPI score as reported by the mothers. Three day care variables were used: whether or not the child was ever in a day care center or preschool in the first 18 months after random assignment, the number of months the child spent in a child care center during those first 18 months post-baseline, and a measure of the number of different child care arrangements the child used during that period — that is, the child care outcomes on which the program impact was most substantial.

The results of these analyses are shown in Table 8.12. For the full sample, the instrumented day care variable was statistically significant, suggesting that use of day care centers increased maternal BPI scores, even after controlling for other maternal characteristics. Among those focal children who were ever enrolled in a day care center at the 18-month point, standardized BPI scores averaged 5.18 points higher than among those who had never enrolled. The center panel indicates that each month the child spent in a day care center was associated with a 0.59 point increase on the standardized BPI scale. Finally, the rightmost panel shows that each occurrence of a *new* child care arrangement during the first 18 months of follow-up was estimated to increase BPI scores by 4.40 points.[29]

The effects of day care use on BPI scores were also examined for various subgroups in this sample. Contrary to research that has found that very young children are most negatively affected by enrollment in a day care center, the results in this study suggest that older children (that is, those who were 18 months or older at baseline) were more likely than younger ones to be adversely affected.[30] Among older children, having spent time in a day care center was associated with BPI scores that were 7.44 points higher than for children without day care experience. The day care effects were similar for males and females, although the effect was statistically significant only for girls. The results for subgroups taking gender and age simultaneously parallel the impact results; the behavior of younger boys and, especially, older girls was significantly affected by day care center use.

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[28] The first stage of this analysis involved regressing day care use on experimental group status. Since children were not randomly assigned to day care use but were randomly assigned to research group status, the results of the first stage can be used to control for possible selection bias in this analysis. (Remember that the term "selection bias" refers to the possibility that women who use child care may be different in unmeasured ways from women who do not. Failure to control for these differences may affect estimates of the effects of child care on subsequent outcomes.) In the second stage of this estimation procedure, predicted values for day care use from the first stage are used, together with other maternal characteristics at baseline, to predict child development outcomes. A more detailed discussion of this procedure is included in Appendix E.

[29] In addition to examining the direct effects of child care on BPI scores, a separate analysis explored the relationship between day care use and the *discrepancy* between maternal BPI scores and teacher BPI scores. This analysis included only children for whom both of these measures were available. Day care use was found to increase the discrepancy between the mother's and teacher's BPI scores, suggesting that at least part of this effect may have been unrelated to the children's actual behavior.

[30] In a separate analysis, however, a variable indicating whether the child had ever been in *any* type of regular non-maternal child care arrangement before age 1 was used as the instrumented variable. This is a variable for which, as shown in Table 8.4, there also was a significant program effect. The regression coefficient for predicted use of child care before age 1 on BPI scores was substantial (22.79) and statistically significant (p = .070).

Table 8.12

Estimated Effects of Child Care in the First 18 Months After Random Assignment on New Chance Sample Members' Maternal Behavior Problems Index Scores (Standardized), at 42 Months After Random Assignment

		Ever in a Day Care Center, Random Assignment to 18-Month Follow-Up			Months in a Day Care Center, Random Assignment to 18-Month Follow-Up			Number of Different Child Care Arrangements, Random Assignment to 18-Month Follow-Up		
Model	Sample size	Regression Coefficient	Standard Error	p[a]	Regression Coefficient	Standard Error	p[a]	Regression Coefficient	Standard Error	p[a]
Full Sample	1,700	5.18	(2.26)	0.022 **	0.59	(0.26)	0.023 **	4.40	(2.08)	0.034 **
Child younger than 18 months at random assignment	965	3.92	(2.93)	0.181	0.43	(0.33)	0.185	3.99	(3.18)	0.210
Child 18 months or older at random assignment	735	7.44	(3.89)	0.056 *	0.90	(0.48)	0.063 *	5.42	(3.18)	0.089 *
Male child	882	6.32	(3.86)	0.102	0.74	(0.46)	0.109	4.71	(3.11)	0.130
Female child	818	4.97	(2.76)	0.072 *	0.57	(0.32)	0.075 *	4.63	(2.84)	0.104
Male child younger than 18 months at random assignment	503	9.87	(5.91)	0.096 *	1.05	(0.65)	0.107	7.39	(5.24)	0.159
Male child 18 months or older at random assignment	379	2.82	(5.66)	0.619	0.39	(0.80)	0.621	2.38	(4.93)	0.629
Female child younger than 18 months at random assignment	462	-0.21	(3.38)	0.950	-0.02	(0.38)	0.950	-0.30	(4.69)	0.950
Female child 18 months or older at random assignment	356	12.89	(6.32)	0.043 **	1.46	(0.76)	0.055 *	7.72	(4.60)	0.095 *
Mother's CES-D (depression) score[b] at random assignment										
0-15 (not at risk)	802	1.84	(3.90)	0.638	0.27	(0.57)	0.640	1.42	(3.04)	0.640
16-23 (at some risk)	431	5.10	(4.26)	0.232	0.47	(0.40)	0.239	5.54	(5.37)	0.303
24-60 (at high risk)	464	10.43	(4.31)	0.016 **	1.05	(0.43)	0.016 **	9.18	(4.96)	0.065 *

continued)

Table 8.12 (continued)

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: This table shows the estimated effects of unit changes in selected child care outcomes on children's behavioral problems reported by New Chance sample members (measured with the Behavior Problem Index). Effects are shown for 1,700 sample members for whom there were 42-month survey data on child care and Behavior Problem Index Scores. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires. The regression coefficients were estimated using a two-stage least squares procedure, as outlined in Appendix E.

Calculations for this table are based on data for the focal child. For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as ***= 1 percent; **= 5 percent; *= 10 percent.

[b] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

The bottom panel of Table 8.12 shows results of these two-stage analyses for subgroups of the sample based on the mothers' depression scores when they applied to New Chance. This table suggests that day care use did not have an adverse effect on children's behavior among children whose mothers had low or moderate depression scores at baseline. Among mothers who were at high risk of depression, however, each month the child spent in a day care center added an additional point to the BPI scale; in this subgroup, children who were in a day care center in the first 18 months after random assignment had BPI scores that were, on average, 10.43 points higher than those who did not use a day care center.[31]

These analyses lend support to the explanation that the program's negative effects on children's behavior were concentrated among women who were at high risk of depression at baseline and were mediated by the program's impact on child care use. The pattern of effects is not entirely consistent with findings from other studies, however, which have generally found adverse effects from nonmaternal child care on younger children and on boys. In this sample, the effects were most pronounced for younger boys — but also for older girls. The reasons for this pattern are not clear.

The fact that day care was found to have negative effects only among women who were highly depressed when they applied to the New Chance programs suggests that it is these highly vulnerable women whose attachment to their children is most jeopardized by placing them in center-based care. Indeed, there is research evidence that links maternal depression to attachment insecurity (DeMulder and Radke-Yarrow, 1991; Lyons-Ruth et al., 1991; Teti et al., 1995). And, in fact, maternal depression is the alternative hypothesized pathway for the impacts on children's outcomes.

2. **Effects of Maternal Stress and Depression on Children's Behavior**. There is extensive evidence that maternal depression places children at elevated risk for an array of behavioral and emotional problems, including childhood psychopathology (Downey and Coyne, 1990; Gelfand and Teti, 1990; Hammen, Burge, and Stansbury, 1990). While various theories have been suggested to explain this link, the most widely accepted explanation centers on the negative effects of maternal depression on parenting behavior and on the mother-child relationship. Depressed women's parenting behavior, in comparison with other women's behavior, has been characterized as less warm and responsive, more hostile and critical, and generally less competent (Goodman, 1992; Harnish, Dodge, and Valente, 1995). It is reasonable, therefore, to speculate that the program's impact on maternal depression and stress (including parenting stress) mediated the impacts on children's outcomes.

An instrumental variables analysis was again used to test the possible mediating effect of the mother's emotional status on standardized BPI scores. In this analysis, improvements to maternal CES-D scores from baseline to the final interview were predicted from baseline characteristics and experimental group status in the first stage of the analysis, and predicted improvements to depression scores were used in the second stage to predict BPI scores. As was true for the analysis with child care mediators, this analytic approach controls for selection (that is, controls for factors associated with different levels of depression, since women cannot be randomly assigned to different well-being statuses).

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[31] Again, analyses comparing maternal BPI and teacher BPI scores found that day care use was a strong predictor of diverging opinions among mothers and teachers regarding children's behavioral problems. Especially among women at high risk of depression, day care use was strongly associated with mothers' rating their children more negatively than did the teachers.

The results shown in Table 8.13 indicate that, for the aggregate sample, maternal depression had the anticipated negative effect on mother-reported BPI scores, and this effect was statistically significant. Every 1-point improvement in CES-D scores was associated with a 1.39 decline in BPI scores, even after controlling for a host of other maternal characteristics. For subgroups of the sample, however, the results are largely nonsignificant — the exception being children who were older than 18 months at baseline.

Although this analysis offers some support for the impact on maternal depression as the hypothesized pathway for the program impacts on children's behaviors, the results are somewhat less persuasive than was true for the child care explanation. The effects shown in Table 8.13 are less powerful and less pervasive than those presented in Table 8.12. Moreover, the fact that the BPI scores may *reflect* maternal depression must be considered — that is, the BPI scores as reported by the mothers might be inflated as a result of the mothers' emotional state. Even though there was some independent corroboration that the children's behavior was different in the two research groups, nevertheless the most noteworthy group differences with regard to child outcomes emerged on mother-reported scales.

Finally, it is important to consider the possibility of reversed causality in the depression-child behavior relationship. In Chapter 6 we explored the possibilities that the impact on maternal depression was the result of frustrated expectations for improved life circumstances or the result of instability in living arrangements. A third possibility concerns the effects of the children's behavior on their mothers' well-being. Researchers have found that the most salient source of stress among low-income mothers with young children was the children's misbehavior and the mother's stressful interactions with the young child; moreover, stress was highest among mothers of preschool-age boys (Olson and Baynard, 1993). Thus, if some aspect of the intervention (such as child care) resulted in more behavior problems among children in the experimental than in the control group, then it is possible that this misbehavior and the ensuing tensions in the mother-child relationship caused higher levels of maternal depression. More than likely, there is some reciprocal causation.

In summary, there is some support for both child care use and maternal depression as the mediating pathways for the unfavorable program impacts on child development outcomes. In all likelihood, both mechanisms played a role — and there are strong relationships between the two. Mothers at high risk of depression at random assignment had the largest 18-month impact of any subgroup on day care center use, and these were the very women for whom the day care effect (Table 8.12) was greatest. Hence, these women experienced the greatest change in this aspect of their lives and in the relationship with their children, while being least prepared to deal with it. Also, these women were more likely to experience other negative program impacts, including the unfavorable impact on improvements to depression scores.

Table 8.13

Estimated Effects of New Chance Sample Members' Improvements to CES-D Scores (from Random Assignment to 42-Month Follow-Up) on the Focal Child's Standardized Behavior Problems Index Score at 42 Months After Random Assignment (as Reported by the Mother)

	Estimated Effect of a Unit Reduction in the CES-D Score on Reported Behavior Problems			
Sample or subgroup	Sample Size	Regression Coefficient	Standard Error	p^a
Full Sample	1,775	-1.39	(0.74)	0.060 *
Child younger than 18 months at random assignment	1,011	-1.63	(1.80)	0.365
Child 18 months or older at random assignment	763	-1.09	(0.65)	0.091 *
Male child	919	-1.75	(1.22)	0.152
Female child	856	-1.17	(0.96)	0.224
Male child younger than 18 months at random assignment	524	-4.01	(5.64)	0.477
Male child 18 months or older at random assignment	395	-0.54	(0.75)	0.471
Female child younger than 18 months at random assignment	487	0.82	(1.89)	0.667
Female child 18 months or older at random assignment	368	-1.72	(1.26)	0.173
Mother's CES-D (depression) score at random assignment				
0-15 (not at risk)	838	-0.72	(1.08)	0.508
16-23 (at some risk)	447	-4.66	(12.50)	0.709
24-60 (at high risk)	490	-1.99	(1.28)	0.120

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: This table shows the estimated effects of unit reductions in depression (measured with the CES D scale) on children's behavioral problems reported by New Chance sample members (measured with the Behavior Problems Index). Effects are shown for the 1,775 sample members for whom there were 42-month survey data on CES-D scores and Behavior Problem Index scores. The regression coefficients were estimated using a two-stage least squares procedure, as outlined in Appendix E.

The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

For the 65 percent of sample members who had one child at random assignment, that child was the focus of all child-related questions on the 42-month survey, and is thus referred to in this report as the "focal child." The focal child for each sample member who had two or more children at random assignment was chosen at random from among those children.

The Behavior Problems Index (BPI) is a widely employed scale used to describe the incidence of behavioral problems of children aged four or older, usually as described by a parent. Raw scores for the BPI and its six subtests were converted to standardized normed scores, which are based on data from the 1981 National Health Interview Survey. These standard scores (with a mean of 100) are standardized separately for boys and girls within single years of age.

[a] The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Chapter 9

Reflections and Implications for Programs and Policy

A very quick reading of the preceding chapters suggests a too simple—and incorrect—conclusion: that the services provided by New Chance were ineffective. The reality is far more complex. Although the New Chance sites did a competent and often an excellent job of providing services, control group members across the sites received many of the same services as experimentals, especially services geared toward human capital development. Furthermore, because of high absenteeism and early departures from the program, experimentals received only small amounts of many of the services prescribed in the program model, especially services geared toward personal development (which in any case were accorded less time in the program schedule than was adult education).

I. A Broad View of the Findings

A. The Positive Side of the Picture: Progress in the Face of Problems

It is important not to lose sight of the fact that as members of the research sample, both experimentals and controls, grew older, their lives were improving in many ways. At the time of random assignment, only 6 percent held a high school diploma or a GED; by the 42-month point, over half the experimentals (52 percent) and 44 percent of the controls had earned one of these credentials, and 25 percent of both groups held a training certificate or license.

Many young women also seemed to be moving toward greater personal stability. Over the course of the follow-up period, the ability of the young women to plan their childbearing rose steadily, as evidenced by the increasing proportions of women who used a prescription method of birth control or who had had tubal ligations. Although at the 42-month point a sizable minority of women (43 percent) had scores on a depression scale indicating that they were at risk of clinical depression, this figure marks a notable decline from the baseline proportion of 53 percent.

The evidence is also clear that many young and poor mothers, with or without the assistance of New Chance, were taking steps to improve their economic situations. Sixty-three percent of sample members did not work at all during the year prior to random assignment; in contrast, over half were employed during the 12 months before the 42-month survey. Indeed, employment rates of women in the New Chance sample were comparable to those of older participants in several recently evaluated welfare-to-work programs. While the majority of sample members (74 percent) were receiving AFDC at the 42-month point, this number represents a considerable drop from the 95 percent on welfare at baseline. It is also worth noting that almost one quarter of this group of sample members reported a total monthly income of more than $1,500 at Month 42.[1]

These achievements are especially notable in view of New Chance sample members' youth and relative inexperience, as well as their responsibility for the care of children who were, for the most part, still quite young. Other barriers often stood in their way as well. The young women interviewed for the New Chance monograph *Lives of Promise, Lives of Pain* (Quint and Musick, 1994) recounted examples of poor supervision and outright racial discrimination in the workplace. Their stories also illustrated how other people—often people they loved—sabotaged their efforts: the mother, herself a high school dropout, who repeatedly told her daughter that she would never amount to anything; the boyfriend who, miffed by an argument, refused to drive his partner to her job; the ostensible friend who defaulted on her share of the rent. These kinds of situations sapped the young women's energies and sometimes their resolve as well. Yet the young mothers were able to move forward even in the face of serious problems that were aggravated by poverty, which left them no cash reserve for emergencies.

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[1] Another quarter, however, had incomes of less than $600.

These data belie the view that young welfare mothers are indisposed to move forward in their lives. The perception that they are content to "sit back and feed at the public trough" is in serious need of revision. The New Chance findings reflect instead the high degree of resilience that many young mothers exhibit and their determination and ability to create futures for themselves and their children that will be better than their pasts.

B. The Negative Side: A Disappointing Impact Story

Nonetheless, with respect to the majority of outcome areas, New Chance had no effect over and above services already available to control group members. Only in the area of GED receipt did young women in New Chance register strong and positive impacts (45.2 percent of the experimentals had earned a GED at the 42-month point, compared with 33.4 percent of the controls); the magnitude of this impact declined between the 18- and 42-month follow-up surveys, as members of the control group also entered and completed adult education (ABE/GED) programs. Furthermore, controls were significantly more likely than experimentals (10.4 percent and 6.9 percent, respectively) to earn regular high school diplomas, which employers generally value more highly than a GED .

Within 42 months after entering the research sample, experimentals and controls were equally likely to have experienced a repeat pregnancy (approximately 74 percent of both groups) and to have given birth (55 percent). They were equally likely to have been employed (approximately 68 percent), and at similar—and low—wages ($5.66 an hour, on average, during months 31-42). At the 42-month point, the two groups were equally likely to be receiving welfare (approximately 75 percent), and their children were similar along most dimensions. Finally, with regard to some outcomes, New Chance had small but statistically significant effects that were in the opposite direction of what had been anticipated; experimentals were more rather than less depressed than controls, and reported more stress.

The results are especially disappointing because of the lack of sustained positive impacts for any subgroups or sites. It is not uncommon for evaluations of programs to indicate that while the intervention was not successful for the majority of participants, it was effective for a particular subgroup or for a particular site. This does not appear to have been the case in New Chance. No subgroup or site did especially well on a sustained basis—that is, across a range of outcome areas. For example, those women in the experimental group who had never worked before entering New Chance were more likely than their control counterparts to have earned a GED or high school diploma, but this gain did not translate into higher earnings, lower welfare receipt, a more positive home environment, or better outcomes for their children vis-à-vis the outcomes for controls. Many sites registered a significant and positive effect on GED receipt, but this outcome was rarely associated with positive economic or child development impacts.

To the extent that a subgroup story exists, it appears to indicate that New Chance was an especially problematic intervention for young women who were at high risk of depression at the outset of the demonstration. It is true that such women were more likely than their control counterparts to have received a GED or high school diploma by the 42-month point, but young women at high risk of depression at baseline were also significantly more likely than controls to report that they frequently felt stressed, and they registered smaller improvements in depression scores than did controls. Moreover, the children of the experimental group members, compared with the children of similarly depressed controls, scored lower on a test of preschool readiness and higher on a measure of behavior problems, as reported by the mothers. Thus, participation in New Chance may actually have worsened the socioemotional status of these women, and it seems to have negatively influenced their perceptions of their children as well.

Taken together, these findings suggest that programmatic solutions to the problems associated with being young, poor, and a mother are not easy to come by, especially when efforts are targeted toward a particularly disadvantaged group of high school dropouts. Certainly the results should give pause to advocates for multiservice, voluntary programs for young mothers on welfare who are high school dropouts.[2] At the same time, they raise questions about the consequences the 1996 welfare legislation will have for women and children. And they indicate ways in which programs may need to improve their offerings in order to be more effective. These issues are considered later in this chapter; in the section that follows, some possible reasons for the impact findings are examined.

C. Understanding the Findings

There are a number of potential explanations for the lack of program impacts and for impacts that are counterintuitive. It is important to state at the outset, however, that no single explanation is likely to pertain across all the outcome areas and that multiple factors are likely to be working in conjunction with each other.

Several potential reasons for the program's limited effects may be cited:

- Members of the control group received more services than had been expected.
- Members of the experimental group received too little of some services to make a difference.
- The program may have produced effects that then led to other, unanticipated side effects.

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[2] As is noted elsewhere in this report, Project Redirection, a comprehensive, voluntary initiative for younger teenage mothers and pregnant teenagers, did have positive effects on employment and child outcomes, but positive impacts were limited to those who were enrolled in school when they entered the program.

- Interpersonal difficulties, adverse economic conditions, cultural influences, and other factors in the young women's environments constrained the young women's ability to make greater changes in their lives.

- The New Chance program model requires modification, or may be inappropriate, at least in some respects and for some part of the population it served.

Each of these explanations is discussed in the pages that follow.

1. **High Levels of Service Receipt by Controls.** In selecting sites to participate in the New Chance Demonstration, MDRC was mindful of the fact that New Chance did not hold a monopoly on the services it offered. Every community had organizations that operated GED and skills training programs. Every community also had other agencies that offered family planning and mental health services. The Demonstration's planners recognized that what was unique about New Chance was not the services themselves but the way in which they were packaged; New Chance brought together a variety of services young mothers were deemed to need and delivered them at a single location. This "one-stop shopping" approach was designed to facilitate access to these services. The role of the New Chance case manager was, in large measure, to monitor participants' receipt of services as well as to help the young women resolve issues that might interfere with program attendance and progress.

MDRC staff anticipated that members of the control group would use these services to some degree, but prior experience provided no reason to think that the *level* of services received by controls would be so high, especially with regard to adult education (ABE/GED) training services. A significantly higher proportion of experimentals than controls (89.2 percent versus 76.2 percent) participated in education programs of some kind, and experimentals also participated significantly longer (37.0 versus 26.4 weeks, on average). The same was true of skills training (with 47.5 percent of experimentals versus 38.1 percent of controls ever participating, for an average of 13.4 weeks versus 10.1 weeks, respectively). Nonetheless, what is most striking is the extremely high proportions of controls receiving these services, higher than in any previous MDRC demonstration whether involving mandatory or voluntary populations. Presumably these levels of participation reflect the young mothers' desire to catch up with their peers who graduated from high school and their vision of a high school diploma or high school equivalency certificate as key to a better future.

The high level of education and training services received by controls reduced the likelihood that New Chance would produce sizable impacts on other outcomes that, according to human capital theory, are associated with education and training. Thus, New Chance had a statistically significant impact on receipt of a GED but, again in comparison with other demonstrations, that effect was not very large.[3] At the same time, controls were more likely than experimentals to earn a high school diploma, generally considered to be more valued by employers than a GED. Analyses in Chapter 7 link receipt of a training credential (a trade license or certificate) to substantial increases in employment rates, hours worked, and earnings, but New

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[3] For example, among young mothers enrolled in JOBSTART, the experimental/control difference in receipt of a GED or high school diploma within 48 months after random assignment was 15.3 percentage points; in New Chance, the impact was 8.1 percentage points within 42 months after random assignment.

Chance did not increase the proportion of experimentals acquiring such a credential above the control group level. It is not surprising, therefore, that at the 42-month point the labor market outcomes of the two research groups were virtually identical.

The explanation that controls received high levels of adult education and training services appears to hold considerable potential to account for the absence of program impacts associated with employment and welfare receipt. With regard to services related to personal development, however, disparities in the proportions of experimentals and controls receiving such services as family planning, parenting education, and life skills classes remain pronounced (although the size of the difference is undercut by the fact that many experimentals who got personal development services received them in only small quantities). Other explanations for the lack of program impacts—or for negative impacts—in the areas of fertility, mental health, and parenting must be sought.

2. Low Levels of Service Receipt by Experimentals. A second reason why New Chance may not have produced the desired effects is that quite aside from the services controls received, members of the experimental group may not have received the program services in sufficient amounts to make a difference. On average, experimentals participated for just under 300 hours in New Chance activities (excluding counseling and college classes); while 22.1 percent of the experimentals received more than 500 hours of services, however, over a third (36.1 percent) received 100 or fewer hours (including 11.5 percent who got no services at all). Both poor attendance and early departures from New Chance account for the relatively low intensity of participation.

Most of the New Chance sites reported problems securing good attendance; on any given day, it was not unusual for half the young mothers officially on the roster to be absent. The young women cited their own illnesses or those of their children, pregnancy-related malaise, problems with child care or transportation, and conflicting medical or welfare appointments as reasons for nonattendance. Program staff members believed that many young women had been out of school for so long (and had been frequent truants even when they were nominally enrolled) that they had fallen out of the habit of following a daily routine. They also noted the prevalence of serious problems and crisis situations: the reality or threat of homelessness (which affected almost half an early group of enrollees) as well as substance abuse and domestic violence. Case managers and other staff members could and did deal with some of these situations; for example, they helped young women find a new place to live when necessary, and on occasion they referred especially troubled young women to other agencies for assistance. But their ability to intervene in other situations—for example, to help resolve disputes with partners—was limited.

Once participants went on to Phase II activities that were held off-site, their problems frequently multiplied. Staff members reported that skills training programs and community colleges were all too similar to the impersonal high school settings in which enrollees had done poorly in the past; they did not provide the sustained support that the New Chance teachers and counselors offered. While case managers' responsibilities included contact with enrollees who had moved on to Phase II activities, the need to resolve the problems of young women still at the program site reduced the frequency and depth of these contacts. The disparity between the proportions of students entering training programs and community college and the proportions who actually received a certificate or two-year degree attest to the difficulties New Chance enrollees experienced in these next-step activities.

In general, retention was a problem for the program sites. While young women were permitted to remain in New Chance for 18 months, on average they were active for only 6.4 months, and 56 percent had left the program by the ninth month after random assignment.

It is not clear how much absenteeism and early dropping out reflect dissatisfaction with the New Chance program. It will be recalled that when enrollees were asked to rate various aspects of the program, their ratings were, on the whole, quite positive. But it also seems plausible that in the face of other pressures and difficulties, attending New Chance was not the highest priority for many young women. This appears to be especially true for those who were the most disadvantaged, economically and socioemotionally, at the outset; in any event, they registered lower participation than other subgroups of experimentals.

In any event, it seems incontrovertible that the combination of shorter than expected length of stay and higher than expected absenteeism produced a level of participation that, on average, was considerably lower than had been planned. In this context, it is useful to reexamine the average hours participants received of each service (see Table 4.2). If (as in many schools) six hours were considered a full learning day, then the 101 hours, on average, that experimentals received of ABE/GED instruction would equate to 17 days. The 67 hours of skills training would be about 11 days, and the 28 hours of work internship and 26 hours of employability development would amount to about 4 ½ days each. Experimentals would also have received, on average, three days or so of parenting education , life skills instruction, and other group activities, and fewer than two days of family planning and health education workshops. These levels of participation may have been just too low to create real changes in participants' lives.

The results of the nonexperimental analyses of the relationship between levels of service and outcomes suggest that the problem may not have been the education and job training services that experimentals received but the fact that they received relatively few of them while controls received more services than anticipated. Those analyses, it will be recalled, indicate that attending more hours of ABE/GED instruction was associated with a higher degree of GED attainment, and that attendance in skills training and college, as well as the receipt of educational credentials, produced positive effects on earnings and other employment outcomes. Furthermore, spending more hours in family planning was associated with a reduction in subsequent childbearing that barely missed statistical significance. In general, it seems likely that at least with regard to some outcome areas, a more intensive program treatment would have resulted in better outcomes for members of the experimental group and presumably for larger between-group impacts as well.

It is tempting to speculate about how the impacts might have been different had New Chance been mandatory—that is, had participants risked losing their welfare grants if they failed to attend. There is no automatic or obvious answer to this question. As is discussed later in this chapter, the program has now become mandatory at some sites; program staff there report that attendance is considerably better than it was during the demonstration, when the program was voluntary. Staff at other sites, however, believe that a mandatory program would mean an angrier, less cooperative group of participants. What the prior research suggests is that programs for young mothers, whether mandatory or voluntary, have had difficulty securing good attendance and that mandatory programs have made extensive use of sanctioning in efforts—sometimes successful, sometimes not—to induce enrollees to show up.

If receiving program services in insufficient amounts is indeed a major cause of attenuated program impacts, then a number of implications follow. One is that programs need to find ways to secure more regular attendance; measures the New Chance sites have undertaken since the end of the demonstration period to accomplish this objective are discussed later in this chapter.

A second implication is that while providing comprehensive services may seem both reasonable and necessary, it is exceedingly difficult to accomplish in practice. In the attempt to do so much, some services will almost inevitably receive less attention than they deserve. Programs may do better to concentrate on delivering a more limited set of services but doing so in a thorough and effective way. As is discussed below, programs offering differentiated services may also need to be targeted toward different subgroups within the young mother population.

3. Unanticipated Side Effects. It seems likely that some impacts produced by New Chance—especially impacts in the opposite direction to what was expected—may have been the unintended consequences of other effects. For example, program staff members helped experimentals to move out of conflict-ridden living arrangements and into their own living quarters. But being on their own may have enabled the young women to live with their boyfriends and may thereby have contributed to the program's ineffectiveness in reducing subsequent pregnancies. More frequent moves on the part of experimentals may also have led to increased stress levels among members of the New Chance group.[4]

At 42 months, mothers in the New Chance group were more likely to be depressed and to be dissatisfied with their standard of living than their control counterparts. These findings suggest that New Chance raised some mothers' expectations for the future but did not then enable them to advance economically, instead leaving them neither comfortable with their current status nor confident that they would be able to improve their situations.

It seems plausible that mothers who were more stressed and depressed were also more likely to rate their children's behavior negatively. If so, the mothers' emotional state would account at least in part for the fact that those in the New Chance group reported more behavior problems on the part of their children than did mothers in the control group. Whether the children of experimentals really did exhibit more behavior problems than the children of controls is uncertain. Teachers' ratings do not strongly corroborate those of the mothers, but neither do they refute them; the teachers tended to evaluate the children of women in the two research groups as quite similar in behavior but also rated children in the experimental group as having more multiple behavior problems requiring parental notification.

If, indeed, children in the experimental group behaved worse than their control counterparts, then the fact that experimental children experienced a larger number of child care arrangements—and hence, more frequent disruptions in these arrangements—may be one of the reasons. Data in

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[4] For the sample as a whole, there was a statistically significant and positive correlation between number of moves since the birth of the focal child and stress; every move was associated with a 2 percentage point increase in the proportion reporting high stress.

Chapter 8 indicate that each entry into a different child care arrangement resulted in a 4.4-point increase in the child's behavior problem score, as reported by their mother.

If, on the other hand, experimentals' children were not really more badly behaved than the children of controls but were only perceived to be so by their mothers, there is nonetheless reason for concern. If mothers expect their children to behave badly and therefore treat them badly, then their children may well exhibit behavior problems or other psychological difficulties in the future.

4. **External Constraints on Positive Change.** Some observers have hypothesized that experimentals confronted adverse circumstances that made it impossible for them to translate their desires into actions and to realize more substantial gains. For example, while the evaluation did not include an analysis of labor market conditions in the 16 New Chance sites, a number of studies suggest that employers are unwilling to hire young people for positions that offer real responsibility and that pay well. Instead they prefer to hire somewhat older employees for these jobs and to "season" young employees in low-paying, high-turnover jobs. Thus, earning a diploma or GED may yield few economic dividends until credential-holders reach the latter part of their 20s. Under such conditions, it may be unrealistic to expect young women on welfare to become self-sufficient, especially in the absence of low-cost child care and universal health insurance.

Social factors may also have resulted in attenuated outcomes. Program operators reported that gains young women made while in the program could be undercut (if not undone altogether) by family members and boyfriends who were unsupportive of, or threatened by, the young women's achievements. At one site, for example, a number of young women were planning to go to a weekend-long college "open house" with New Chance staff members; one by one, the participants' mothers reneged on their promises to provide help with child care while their daughters attended the event. Boyfriends could easily thwart fertility control efforts by refusing to use condoms, and program directors at several sites estimated that over half their participants had been the victims of domestic violence at some point in their lives.

In short, New Chance participants were not isolated from external factors; the interpersonal and economic environments in which they lived acted as constraints on their ability to achieve program goals. In this regard, it is instructive to compare the program's positive impacts on GED attainment—an area over which participants *did* exercise a relatively high degree of control—with its inability to affect other domains. Such a comparison suggests that the areas in which the young women's ability to control their own destinies was especially limited (such as employment and family planning) were also among the areas in which the program's effectiveness was most limited.

D. Re-examining the Program Model

The disappointing results suggest a need not only to modify and improve implementation of the program model but to question the model itself.

1. **Ongoing Change at the New Chance Sites.** The twelve New Chance sites that continue to operate have undergone continual evolution and change, as program staff members identify ways of responding to the implementation problems they have encountered and to participants' needs. Site personnel note that the New Chance evaluation covers a period when sites were on the steep slope of a learning curve; they maintain that since then they have modified

existing practices and adopted new ones. In general, they believe that their programs are much more solid than in the past. But, they add, they are still learning and growing and changing.

Innovations adopted by individual programs are discussed in Appendix F. Here, attention focuses on the general directions these changes have taken.

One of the most important modifications since the demonstration period is an increased emphasis on Phase II activities. Many sites report that they now impress upon enrollees from the beginning of their stay in the program that the purpose of New Chance is eventual employment. As one program director put it, the focus of the program has shifted from literacy to work. Several sites have also taken steps to ensure that participants in Phase II activities receive more active case management assistance than was the case during the demonstration phase—an issue discussed at greater length later in this chapter. At least one site now employs a staff member specifically charged with job placement. It is also noteworthy that one program has supplemented its GED preparation classes with an alternative high school program for young women who can obtain a high school diploma within a reasonable time period.

Sites have also tried to strengthen their family planning message. Medical personnel outstationed at one program routinely administer Depo-Provera injections to young women who want to use this convenient, long-acting contraceptive method.

A number of sites have also supplemented their programs by expanding their counseling resources. Some have arranged for participants to be referred to local mental health agencies; one local program, which at the outset of the demonstration employed one psychologist part-time, now employs five, and the project director estimates that 75 percent of participants receive mental health services. Some local programs have also increased the range of topics that on-site counseling services cover to include domestic violence, for example. Some case managers have conducted joint counseling sessions with participants and their boyfriends and believe that this approach makes for increased success.

Participation in New Chance was mostly voluntary throughout the demonstration period. Subsequently, several sites serve women who are mandated to participate through JOBS or, in some instances, by the courts. Staff members at these sites report that mandates, with their accompanying threat of welfare grant reductions, have had a salutary effect on attendance (although there is by no means a consensus among staff across the various programs about the value of mandates).

2. Reassessing the Program Model. It is also possible that the New Chance model itself needs to be rethought from the ground up, beginning with its "one size fits all" approach. New Chance was developed for a population that, in comparison with other programs serving a welfare clientele—and even other programs serving teen mothers—appeared to be relatively homogeneous, comprising as it did young women in a fairly restricted age range (16 to 22) who had left high school before graduating. The program model was grounded in the assumption that all of the program participants would need (or would at least benefit from receiving) an array of education, training, parenting, health, family planning, life management, and other services and that New Chance should therefore provide all these services to all enrollees.

With the advantage of hindsight, it is possible to question both the assumption of target group homogeneity and that of common needs and, concomitantly, the wisdom of a uniform program. New Chance may have had limited impacts because it did not respond adequately to the diversity of its clientele nor to their felt needs.

The demographic similarities of New Chance research sample members cloak great variation in the kinds of personal and social resources upon which the young women could draw. Some of the young mothers had relatively secure academic skills, supportive families and boyfriends, and a high degree of motivation and inner strength. Others possessed low academic skills, had little in the way of social support, or were severely depressed.

The subgroup data presented earlier in this chapter suggest that New Chance may have been especially inappropriate for young women who entered the program with extremely high levels of depressive symptoms. These young women may need a more therapeutically grounded treatment, either before joining New Chance or in conjunction with other New Chance services. While delineating a more suitable program for highly depressed young mothers is beyond the scope of this report, it seems clear that New Chance did not serve them—or their children—particularly well.

While the New Chance model was developed in consultation with program operators and academicians who were knowledgeable about teenage pregnancy, young mothers themselves did not have input into its design. One wonders how the program would have been different if such input had been obtained. Since many young mothers did not see themselves as particularly in need of parenting classes (although they often wanted help with particular problems, such as discipline), the parenting component might have been eliminated or at least much modified. Other topics might have received a greater or lesser degree of emphasis in response to participants' felt needs.

It is also possible that the real need is for a more flexible and differentiated intervention altogether. College academic programs, directed primarily toward students who are almost exactly the same age and at the same developmental stage as New Chance participants, generally entail a mix of required and elective courses, presumably to ensure some very basic framework of common knowledge while allowing opportunities for students to pursue their own interests. Perhaps a program more individualized and more in line with what each participant felt she needed would have been better attended and would have had stronger impacts.

Even if the underlying view of the young women is accurate and the young mothers really needed all the services New Chance provided, two questions remain: Were the young women able to incorporate and put to good use all the services they received? And could the program build in such a wide array of services while still providing an adequate amount of each one? The impact results suggest that for most young women and most sites, the answer to both questions was "no."

Finally, programs like New Chance that seek to alter the life chances of individual poor people might be more effective if they were embedded in initiatives to improve the economic prospects and enhance the political power of the communities in which these people live. Under these circumstances, individuals could benefit from the new opportunities available in their communities, and communities could draw upon the efforts and motivation of individual residents.

II. Implications of the Findings

A. Implications for Program Practice

Some observers may argue that the New Chance model needs a complete overhaul; others may believe that the model is fundamentally sound but that specific aspects of it should be modified and improved. In either event, the findings suggest a number of measures that programs for disadvantaged young mothers may want to consider in order to serve their clients more effectively. It is worth remarking that, as noted in an earlier section of this chapter, many of these measures have already been adopted by the New Chance sites. It is also important to note that these measures do not necessarily require an infusion of additional resources; rather, they may involve more judicious use of resources that are already available or a shifting of resources from one purpose to another.

1. **Enunciation and Enforcement of Attendance Standards.** Programs invite greater absenteeism if they fail to articulate clear attendance standards. They also need to reward good attendance, follow up quickly on absentees, and penalize consistently poor attenders (ultimately by termination from the program, if need be). The New Chance sites that did all these things did not necessarily have good attendance, but those that *did* have good attendance also had clear rules and expected students to adhere to them.

2. **Consistent Follow-up of Enrollees in Off-site Activities and in Employment.** New Chance had its strongest positive impact in the area of GED attainment, but experimentals' greater success seemed to end once they had that certificate in hand. Despite the higher proportion of experimentals than controls that entered skills training, comparable percentages in both groups actually earned a training certificate or vocational license. Yet the data in Chapter 7 indicate that earning a skills training credential was associated with higher earnings, while merely obtaining a GED was not.

This finding suggests that New Chance and similar initiatives need to do more to ensure the success of participants who have left the program premises for skills training programs or for college. New Chance case managers were supposed to maintain continued contact with and provide ongoing assistance to enrollees who had received a GED and moved off-site into Phase II activities. But, not surprisingly, responding to the needs of participants who were still on the program premises—and therefore better able to capture the case managers' attention—generally took precedence over monitoring the status and well-being of off-site enrollees. Even when resources are scarce, it seems advisable to hold some in reserve (perhaps in the form of case managers especially assigned to participants in off-site activities) to ensure that gains made by participants while they are on the New Chance premises are not lost thereafter.

Follow-up with off-site participants needs to involve more than a cursory check-in. Case managers noted that the young women were often reluctant to report problems until these had reached crisis proportions. Case managers must therefore be willing to probe into many areas of participants' lives and to ask as many questions as are needed to ensure that potential problem areas are not being overlooked.

Programs also need to offer greater assistance to participants not only in finding employment but also in maintaining it. The data in Chapter 7 indicate that job turnover among sample members was a common occurrence. *Lives of Promise, Lives of Pain* explores some of the reasons for that turnover: both unfair and discriminatory treatment on the part of employers and inappropriate expectations and behavior and interpersonal conflicts on the part of the young mothers. Case managers need to help clients better understand their rights and responsibilities as employees so that potential conflicts can be defused before they explode.

3. Development of Work Experience and Employment as Alternatives to College and Training. Even with improved follow-up of participants in Phase II activities, post-secondary education or skills training may not be the best path for many of them. The lack of success of many New Chance enrollees in post-GED training programs and the high rate of college dropout suggest that employment, either in work experience positions or in regular jobs, would be a better option for some young women. Toward this end, programs may need to bolster and expand job development and placement efforts by hiring skilled and experienced job developers and by working with all enrollees who seek job placement assistance (not just those who have completed a GED or training, as was sometimes the case in New Chance).

4. Strengthening of Family Planning Services. Postponing pregnancy is much more than an issue of contraceptive knowledge and availability, but the latter is a precondition of the former. For most young women in New Chance, a subsequent pregnancy was an unplanned event; and at the 42-month follow-up, most young women did not want to have another child for several years, if ever. Given their interest in long-term deferral of pregnancy, it makes sense for program staff to become knowledgeable about and encourage the use of longer-acting contraceptives like Depo-Provera and NORPLANT®. A further advantage of these methods is that, unlike the condom, they are entirely within the woman's control.

Continuous in-service (as well as initial) staff training may be an important tool for ensuring that staff members feel comfortable and knowledgeable in talking about sexuality, so that they do not bypass or give insufficient attention to this issue in meetings with participants. If, despite such training, a case manager remains ill at ease, then responsibility for counseling participants about family planning and monitoring their contraceptive practice should be shifted to another staff member who is more comfortable in this area.

For young women to use contraception regularly, they must be motivated to do so. Program enrollees need to have a clear, consistent, and regularly repeated message about pregnancy postponement. In interventions like New Chance, young women's maternal roles are recognized and celebrated, and it is easy for enrollees to hear mixed messages about the importance of deferring pregnancy. These messages can be made more consistent if the emphasis is on creating a better future for the children young mothers already have by deferring subsequent childbearing until a later point.

5. Recognition of the Importance of Family Members, Partners, and Peers. New Chance enrollees do not exist as isolated individuals; they are enmeshed in social networks whose members exercise enormous influence over their choices and behavior. Thus, decisions about contraception may be influenced by a boyfriend's desire for a child or by a mother's desire for a grandchild. Decisions about work may be affected by what a young woman's best friend or boyfriend thinks about whether work is appropriate for women with young children. Program messages about child-rearing may well conflict with the opinions of a grandmother or other relative.

Programs like New Chance need to reach out to these "significant others," to neutralize potential opposition to program messages where possible and to enlist the support of these individuals in fostering the young women's progress. At the same time, they need to recognize that not all conflicts can be successfully mediated. Under such circumstances, they will need to make the young mothers fully aware of the options they have and the consequences of decisions they make.

6. **Responses to Mental Health Problems and to Domestic Violence.** Staff of programs for young mothers need to be aware that depression is common in this population and may constitute a barrier to full participation and continued progress. Staff members also need to familiarize themselves with the symptoms of this problem and the resources that exist in their communities for treatment. The New Chance sites administered the CES-D to respond to the needs of the research rather than as a diagnostic tool; but programs may wish to use the CES-D or another screening instrument to identify women who would benefit from counseling or from medication.

Program staff also need to be aware of domestic violence in the young women's lives. The prevalence of this problem is uncertain, with staff estimates ranging from 15 to over 50 percent of participants experiencing it at some point. But again, staff members need to be aware of the ways in which violence, or the threat of violence, interferes with participants' ability to attend regularly and to pursue the goals New Chance espouses. They also need to help enrollees realize that they do not somehow "deserve" to be victims and that they can take measures to break away from their victimizers.

One final point: Experimentals exhibited higher levels of both general and parenting-related stress than did controls. Stress need not be seen as a wholly bad thing; it may also be a sign that the program is working, that it is providing an impetus to change and provoking enrollees to think and act differently. The difficulty arises when participants take out their stress on their children or when stress inhibits their ability to make good decisions. The challenge programs face is not to eliminate the stress participants experience—that is likely to be impossible in any event—but to avoid increasing it unnecessarily (for example, by facilitating changes in living arrangements that are likely to be only temporary), seeing that undue pressure does not fall on those who are already most vulnerable, and ensuring that young women have the other supports they need to cope with growth and change.

B. Some Policy Questions

The New Chance findings need to be considered in light of the policy context in which they have unfolded. That policy context has undergone major changes since the inception of the demonstration and continues to evolve in new directions. As is discussed in Chapter 1, recently enacted federal welfare reform legislation, the Personal Responsibility and Work Opportunity Reconciliation Act, gives states primary responsibility for designing welfare policy. The legislation also creates the Temporary Assistance for Needy Families program to replace AFDC and sets a lifetime maximum of five years on federally assisted welfare payments for most adult recipients; states may opt for much shorter time limits if they so chose, or they may continue to support families in need, using state funds after federal assistance had expired. In order for states to receive their full share of federal funds, they have to require increasing percentages of recipients to participate for 30 hours a week in unsubsidized or subsidized employment, on-the-job training, work experience, or community service, or (under very limited circumstances and for no more than 12 months) in skills training. Mothers under age 19 without a high school diploma or GED are required to attend high school or an alternative education program. Recipients who have not secured a job by the end of the time limit will face different options, depending on the state in which they live; some may be cut off welfare entirely, some may be placed in public service employment, some may be in the minority of cases exempted from the time limit, while some may continue to receive assistance financed entirely through state sources. The new legislation requires states to mandate that unmarried mothers under age 18 live with their parent(s) or another adult or in an adult-supervised setting, as a condition of receiving benefits, except under unusual circumstances. It also permits states to impose "family caps" (that is, to deny additional benefits to children born after their mothers go on welfare).

Even before formal devolution was written into the federal statute, states used the waiver process to experiment with welfare in fundamental ways. For example, by October 1996, 31 states had adopted some kind of time limit on welfare receipt; depending on the state, reaching the time limit prompted termination of benefits, imposition of a work requirement, or reduction in the grant amount. Many states also used waivers to impose family caps and to require young teenage mothers on welfare to live with their families or other adults.

Some of the waiver initiatives are now being evaluated, but it is too early to assess their impacts. In the meantime, the experiences of New Chance and of other demonstration programs for young mothers raise critical questions and suggest important cautions about what welfare agencies are likely to encounter and what outcomes they can reasonably expect as they go about implementing the new legislation.

1. What Level of Ongoing Participation Can Be Expected in Education and Work Programs? As has been noted, the 1996 legislation requires young mothers on welfare who are under age 19 to attend high school or an alternative program if they do not already have a diploma or a GED. Older recipients are required to work instead. As the data in Chapters 4 and 7 suggest, when participation is defined as ever attending a day of school or holding a job for a day, then relatively high levels of participation are found among young mothers in both voluntary and mandatory programs. (In the Teenage Parent Demonstration, for example, 66.4 percent of those in the control group, who were under no obligation to do anything, took part in school, job training, or employment within 24 months after program entry.) These findings are a testament to the fact that many young welfare mothers actively take steps to change and improve their lives.

From a policy perspective, the problem may be less the young mothers' lack of initial effort than the lack of consistency and continuity in that effort and its frequent interruption. The New Chance data on absenteeism, early termination, dropping out of college and training, and job turnover all speak to this issue and suggest its pervasiveness in many spheres of activity. Pregnancy, of course, is often a concomitant and sometimes a cause of interrupted progress.

Will time limits add urgency to young women's efforts, increasing their commitment and persistence? Will such limits induce them to make the transition from adolescence to responsible adulthood more quickly than they would otherwise? The answers are not clear, but both the LEAP and the Teenage Parent Demonstration findings suggest that many young mothers in strict mandatory programs will not participate continuously in program activities and that they will incur repeated sanctions for failure to do so.

2. **What Might Be the Effects of a Family Cap?** The impacts both of New Chance and of other demonstration programs for young mothers suggest that fertility-related behaviors are among the most difficult for programs to change. No intervention has proved capable of curbing high rates of repeat childbearing among young women who have already had one child, in part because children, even when unplanned, are valued, and in part because the young women's partners are often unwilling to use condoms and may discourage contraceptive efforts altogether.

Whether financial disincentives for having additional children will result in fewer additional births is very much an open question; perhaps the best guess, based on New Chance and other studies, is that it is unlikely to have large effects and that many children will continue to be born to mothers receiving welfare. Family caps mean that these children will be growing up in environments characterized by even fewer resources than their siblings had at birth.

At the same time, new and compelling evidence indicates that poverty has serious negative consequences for children's cognitive development, especially when poverty is deep and exists during a child's first few years of life (Duncan et al., 1996). States have the option to impose family caps. Policy makers will need to decide whether delivering a message about social values (as family caps are intended to do) is worth placing more children at increased risk of developmental delays.

3. **What Will Happen to Those Who Lose Their Private-Sector Jobs, Especially if They Have Exceeded the Time Limits?** The majority of sample members were employed at some point during the follow-up period. But job-holding was frequently accompanied by rapid job loss. The majority of jobs ended when sample members left them, reporting reasons that included conflicts with supervisors, the lack of good child care, pregnancy, transportation problems, and moves.

It is possible that in the new welfare environment job turnover will be reduced, since welfare will be less attractive and work may be more so. If quick job loss remains the norm, however, policy makers at the state level will need to grapple with the question of whether to make temporary state-funded assistance available to those who lose their jobs after they have passed the time limit and if so, under what conditions. Should such assistance be extended to those who are fired from their jobs? What about those who leave their jobs—sometimes for good reasons, sometimes for reasons that reflect their immaturity and lack of judgment? And what about those who accept jobs that are seasonal or otherwise temporary?

4. **How Will Welfare-to-Work Programs be Staffed?** The difficult psychological and interpersonal problems faced by many young mothers suggest that staff members of welfare-to-work programs need counseling expertise and experience as well as knowledge about mental health resources to which clients with especially serious problems can be referred. The findings on job turnover indicate that many young women need thoughtful, interpersonally skilled supervisors if they are to succeed in community service employment programs.

5. Should Mothers of Very Young Children be Required to Take Part in Welfare-to-Work Efforts? In New Chance, high levels of participation in school, training, or work were associated with high levels of child care utilization—and with high costs. Most New Chance sites offered on-site care and otherwise encouraged use of day care centers because of their reliability and potential for stimulating cognitive development; thus, New Chance is not an ideal test of how much young mothers will rely on such care in order to participate in welfare-to-work initiatives or employment. The data suggest, however, that it would be expensive to provide market care, especially given the high cost of infant care and the large proportion of young mothers with children under age 1.[5] Moreover, there is reason to believe that the more frequent changes in the child care arrangements of experimentals impeded their children's development.

Under the Personal Responsibility and Work Opportunity Reconciliation Act, states may exempt mothers with children under a year old from engaging in the 30-plus hours a week of work and related activities that count toward meeting the state's work participation requirement. The New Chance data suggest that states may want to exercise this option because of both the probable high cost and the potential instability of child care for these children.

6. Is Self-Sufficiency a Realistic Goal for Young Mothers? The recently enacted welfare reform legislation appears to be based on the assumption that by the end of the period covered by the time limit, welfare recipients should be able to attain permanent self-sufficiency. The New Chance results challenge this assumption.

Although young mothers in the New Chance sample worked at surprisingly high rates, considering the fact that they had young children, they held jobs for relatively brief periods, and their employment histories were consequently episodic. A substantial proportion (about 30 percent) of the women who had been employed but were not working at follow-up reported that they had lost their last job either because they had been laid off or because the job itself ended—that is, for reasons that had little to do with their ability or willingness to work.

The average hourly wage of those who worked increased over time, from $4.86 during months 7-18 of the follow-up period to $5.66 during months 31-42. The case has been made that mothers who receive welfare should not be expected to work more than other mothers of young children—and full-time employment is the exception rather than the rule for women with preschool-age children.[6] But even if mothers in the New Chance sample worked at $5.66 an hour for 40 hours a week, 52 weeks a year, their earnings would still leave them more than $1,000 below the 1996 poverty guidelines for a family of three. Earnings at this level do not allow workers to amass a cash reserve to tide them over in periods of unemployment or in case of medical emergencies. At the same time, fewer than one quarter of employed experimentals reported that their current or most recent jobs offered sick days or medical insurance. Without a safety net of the kind provided by government welfare programs, workers must be able to weave their own safety nets if they are not only to become but also to remain self-sufficient. The very conditions of low-wage employment make it exceedingly difficult to create these safeguards against slipping back into abject need.

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[5] The average cost of the onsite care provided by the New Chance program was $420 per slot per month.

[6] As of March 1995, only 42.1 percent of women with children under 6 years old were working full-time. See Jacobs (ed.), page 103.

In short, aside from the obstacles to steady work over which young mothers have some control, the characteristics of the jobs that young, relatively unskilled women are able to obtain—the wages they pay, the benefits they offer (or do not offer)—make it unlikely that these jobs will offer a route to self-sufficiency, especially within a time limit of two years or so.

7. **What Strategies Other than Welfare-to-Work Programs and Other Interventions Focused on Individuals Are Needed to Reduce Poverty and Its Effects Among Young Women and Their Children?** Programs like New Chance that are focused on individuals who are poor (and, in the case of New Chance, are in addition young mothers on welfare) can have an important role in helping participants move forward in their lives. That New Chance itself was of limited success in this regard is not a condemnation of all such programs, especially since New Chance controls received many of the same services as treatment group members. A large number of studies point to the effectiveness of welfare-to-work initiatives, especially ones centered on mandatory job search (see, for example, Freedman and Friedlander, 1995; Friedlander and Burtless, 1995; Gueron and Pauly, 1991).

While many of these interventions have increased employment and reduced welfare receipt, however, they have done little to reduce poverty for most participants. This fact points to the need for other policy approaches that transcend individuals and that transcend the welfare system as well. Some of these policies center on job creation, to ensure that all who want to work (and, in the new environment created by welfare reform, *must* work) have suitable employment. If, as the literature suggests, poverty is the direct cause of negative child outcomes, other policies are needed to ensure that work lifts families out of poverty: low-cost child care, low-cost (or no-cost) universal health insurance, and income supplements in the form of the Earned Income Tax Credit (or, for those who remain on the welfare rolls, continued welfare supplementation of low earnings). In short, a number of social policies are needed if work effort is to yield not just freedom from dependency but also freedom from poverty.

Appendices

Appendix A

A Comparison of Research Groups

This appendix contains an assessment of the effectiveness of the experimental design of the New Chance evaluation. It compares the baseline characteristics of experimentals with those of control group members and measures the statistical significance of apparent differences. On the basis of this comparison, it is concluded that there are no systematic or statistically significant differences between experimentals and controls, as was intended by the research design.

As is discussed in Chapter 2, random assignment was incorporated in the research design of the New Chance evaluation to create a valid counterfactual for the experiences of sample members who were exposed to New Chance. Random assignment is used to create experimental and control groups that are balanced on all baseline characteristics, measured and unmeasured, that may affect relevant outcome measures. As a result, any differences between the two groups that are found after random assignment can be attributed to the program.

A systematic comparison of baseline characteristics can be used to verify that random assignment indeed succeeded in creating two balanced research groups. Table A.1 compares selected baseline characteristics for experimentals and controls. Statistical tests were performed to evaluate the statistical significance of experimental/control differences on the various baseline measures. As expected, differences were generally small and in most cases not statistically significant.

It is difficult to assess the overall effectiveness of random assignment from a broad set of bivariate comparisons such as featured in Table A.1, because the large number of t-tests is likely to generate differences that are "statistically significant" by chance.[1] Also, the baseline characteristics that underlie the statistical tests may not be entirely independent of one another. As a result, one significant difference in Table A.1 may generate another.

To address these problems, a multivariate analysis was used to measure the differences between research groups in one statistical procedure. This procedure tests the hypothesis that experimentals and controls are drawn from the same population by attempting to discriminate between the two groups using baseline characteristics. The actual test is a joint F-test for the significance of a set of coefficients in the following regression equation:

$$STATUS = \beta_0 + \sum_x \beta_x X_x + \varepsilon$$

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[1] This is often referred to as a "multiple comparisons problem."

Table A.1

Selected Characteristics of the New Chance Sample at Random Assignment, by Research Group

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p^a
Demographic characteristics				
Age (years) (%)				0.650
16	1.9	2.1	1.9	
17	18.1	16.1	17.4	
18	22.2	22.2	22.2	
19	25.2	27.0	25.8	
20	19.6	19.6	19.6	
21	10.7	11.7	11.0	
22	2.3	1.3	2.0	
Average age (years)	18.8	18.9	18.8	0.708
Ethnicity (%)				0.505
Black, non-Hispanic	51.7	53.8	52.4	
Hispanic	23.1	22.3	22.8	
White	22.7	22.3	22.5	
Other	2.6	1.6	2.3	
Marital status (%)				0.260
Never married	90.7	89.2	90.2	
Other	9.3	10.8	9.8	
Number of children (%)				0.549
1	65.6	63.7	65.0	
2	26.0	28.2	26.7	
3 or more	8.3	8.0	8.2	
Average number of children	1.4	1.5	1.5	0.811
Age of youngest child (years) (%)				
Less than 1	53.7	52.5	53.3	0.887
1	27.1	28.0	27.4	
2	11.6	11.2	11.5	
3 or older	7.6	8.3	7.8	
Average age of youngest child (years)	1.2	1.2	1.2	0.370
Age at first child's birth (years) (%)				0.971
14 or under	5.4	4.9	5.2	
15	11.8	12.7	12.1	
16	23.3	22.9	23.2	
17	26.4	26.1	26.3	
18	20.8	21.8	21.2	
19	12.1	11.7	12.0	
Average age at first child's birth (years)	16.8	16.8	16.8	0.942
Living arrangement				
Living with (%)				
Mother	34.9	33.1	34.3	0.424
Father	8.4	8.1	8.3	0.833
Spouse or partner	11.0	11.1	11.0	0.970
No other adult	32.8	31.6	32.4	0.596

(continued)

Table A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p[a]
Lived in a female-headed household at age 14 (%)	47.7	49.7	48.3	0.681
Lived with both parents at age 14 (%)	23.1	22.9	23.0	0.921
Education characteristics				
Highest grade completed (%)				0.985
7th or below	2.7	2.7	2.7	
8th	10.3	11.4	10.7	
9th	23.1	22.7	23.0	
10th	30.7	30.7	30.7	
11th	27.7	27.4	27.6	
12th	5.4	5.2	5.3	
Average highest grade completed	9.9	9.8	9.9	0.672
Had high school diploma or GED (%)	6.4	5.3	6.1	0.322
Left school before first pregnancy (%)	37.7	37.4	37.6	0.890
Average number of years since last attended school	2.4	2.4	2.4	0.488
Reading level[b] (grade equivalent) (%)				0.642
4th grade or below	8.7	9.2	8.8	
5th grade	5.3	6.2	5.6	
6th grade	9.9	7.4	9.1	
7th grade	11.0	10.1	10.7	
8th grade	14.5	14.6	14.5	
9th grade	20.8	22.2	21.2	
10th grade or above	29.8	30.4	30.0	
Average reading level[b] (grade equivalent)	7.6	7.7	7.6	0.718
Desired educational attainment for self (%)				0.704
High school diploma or GED	33.9	32.0	33.3	
1-3 years of college (A.A. degree)	30.4	32.1	31.0	
4 years of college (B.A. degree)	21.3	21.8	21.4	
Graduate degree	10.7	11.3	10.9	
Other	3.7	2.8	3.4	
Desired educational attainment for child[c] (%)				0.330
Elementary school	0.1	0.4	0.2	
High school	21.9	21.3	21.7	
College/post-secondary	56.8	57.5	57.0	
Graduate school	21.2	20.7	21.1	
Mother has high school diploma or GED (%)	52.1	52.7	52.3	0.545
Mother attended college (%)	25.4	25.7	25.5	0.448
Father has high school diploma or GED (%)	41.4	45.8	42.8 **	0.017
Father attended college (%)	15.8	17.1	16.2	0.258
Both parents have high school diplomas or GEDs (%)	27.8	31.1	28.9 *	0.057
Both parents attended college (%)	7.1	8.7	7.7	0.300

(continued)

Table A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p[a]
Employment and welfare receipt				
Number of jobs ever held (%)				0.852
0	21.2	21.2	21.2	
1-2	33.7	32.6	33.4	
3 or more	45.1	46.2	45.5	
Average number of jobs held	2.6	2.7	2.6	0.473
Employed at random assignment (%)	2.6	3.8	3.0	0.153
Number of months employed in prior 12 months (%)				0.996
0	63.4	62.9	63.3	
3 or less	18.7	19.0	18.8	
4-6	10.2	10.1	10.1	
7-12	7.7	7.9	7.8	
Prior-year earnings (%)				0.608
$0-$500	79.9	80.8	80.2	
$501 or more	20.1	19.2	19.8	
Length of longest job (%)				0.910
Never employed	20.6	20.6	20.6	
Less than 1 month	4.3	3.4	4.0	
1-3 months	23.3	22.5	23.0	
4-6 months	22.5	23.4	22.8	
7-12 months	17.8	17.8	17.8	
Over 1 year	11.5	12.4	11.8	
Mother employed (%)				0.723
Yes	49.6	48.3	49.2	
No	42.0	43.4	42.5	
Don't know	4.0	4.6	4.2	
Deceased	4.4	3.7	4.2	
Father employed (%)				0.799
Yes	44.6	46.9	45.4	
No	20.5	19.2	20.1	
Don't know	25.1	24.3	24.8	
Deceased	9.8	9.6	9.7	
Receives AFDC (%)				0.531
Own grant	87.1	88.2	87.5	
Other person's grant	7.9	6.5	7.4	
Not receiving AFDC	5.0	5.3	5.1	
Receives (%)				
Medicaid	86.9	89.3	87.7	0.269
Food stamps	83.8	83.8	83.8	0.616
Public housing	22.9	25.2	23.6	0.198
Income from a job	3.0	3.6	3.2	0.390
Family received AFDC when sample member was growing up (%)				0.144
Never	37.1	34.7	36.4	
2 years or less[d]	18.5	18.2	18.4	
More than 2 years, but not always[d]	27.1	31.9	28.7	
Always	17.2	15.2	16.6	

(continued)

Table A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p[a]
Fertility-related characteristics				
Number of pregnancies (%)				0.039
1	43.7	42.0	43.1 **	
2	30.2	36.0	32.1	
3	16.5	15.3	16.1	
4	6.9	4.6	6.2	
5 or more	2.6	2.1	2.5	
Average number of pregnancies	2.0	1.9	1.9	0.206
Ever had an abortion (%)	23.9	22.5	23.4	0.457
When next child is expected (%)				0.965
Not expecting another child	64.2	64.5	64.3	
Within 2 years	1.6	1.8	1.7	
In 2-4 years	17.1	16.3	16.8	
In 5 years or more	17.1	17.4	17.2	
Average number of years until next child is expected[d]	4.4	4.5	4.4	0.842
Current birth control use (%)				
Yes, using birth control	62.0	60.4	61.4	0.857
No, not using birth control	12.4	12.6	12.4	
No partner/not having sex	25.7	27.1	26.2	
Used birth control at last intercourse (%)	70.1	70.6	70.2	0.822
Relations with child's father				
Speaks with child's father[e] (%)	67.3	67.3	67.3	0.999
Has child support order[e] (%)	28.1	26.9	27.7	0.549
Prior and current service receipt				
Ever in occupational skills training (%)	22.6	22.5	22.6	0.952
Services received in the 60 days before random assignment (%)				
Health care for child	85.0	84.5	84.8	0.764
Family planning	24.4	21.5	23.5	0.141
Mental health	3.0	2.2	2.8	0.313
Health care for self	60.1	56.4	58.9	0.109
Parenting	11.2	10.6	11.0	0.679
Life skills	3.2	3.0	3.1	0.829
Counseling	4.2	3.6	4.0	0.478
Other services	10.6	10.7	10.6	0.898
No services	8.2	8.4	8.3	0.885
Has regular child care[f] (%)	45.6	42.5	44.6	0.183
Psychosocial characteristics				
CES-D (depression) Scale[g] score (%)				0.268
0-15 (not at risk)	47.9	44.1	46.6	
16-23 (at some risk)	24.8	26.3	25.3	
24-60 (at high risk)	27.3	29.6	28.1	
Average CES-D score[g]	17.9	18.7	18.1 *	0.079

(continued)

Table A.1 (continued)

Characteristic and Subgroup at Random Assignment	Experimentals	Controls	Full Sample	p[a]
Average number of sources of emotional support	2.8	2.7	2.8	0.200
Average level of satisfaction with emotional support[h]	4.2	4.1	4.2	0.780
Average self-esteem score[i]	38.4	38.2	38.4	0.513
Average Locus of Control score[j]	22.0	22.0	22.0	0.940
Other				
Has home telephone (%)	84.2	82.3	83.6	0.264
Has driver's license (%)	27.9	26.9	27.5	0.642
Sample size	1,401	678	2,079	

SOURCE: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] A t-test or F-test was applied to each difference in characteristics between research groups. The column labeled "p" is the statistical significance level of these differences: That is, p is the probability that these differences exist only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[c] When a sample member had more than one child, her response refers to her first child.

[d] The family's AFDC receipt may not have been continuous.

[e] Includes only those sample members who expected to have more children.

[f] Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

[g] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[h] Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

[i] The measure of self-esteem used was the Rosenberg self-esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

[j] The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

where *STATUS* is the experimental dummy, β_0 is an intercept, X_x a baseline characteristic, and $\in$ an error term. Table A.2 shows the results of an estimation of this equation, using ordinary least squares. In this equation the X_x vector was represented by the same 51 baseline characteristics that were used as covariates in the impact regressions done for this report. The F-test at the bottom of this table shows that the R^2 is not significantly different from zero, implying that there is no systematic relationship between sample characteristics at baseline and the experimental assignment variable. This finding, in turn, suggests that random assignment was effective.

Table A.2

Estimated Regression Coefficients for the Probability of Assignment to the Experimental Group

Variable[a]	Parameter Estimate	Standard Error	p[b]
Constant	0.677 **	0.056	0.000
Allentown	-0.059	0.064	0.360
Bronx	-0.042	0.062	0.501
Chicago Heights	-0.037	0.074	0.615
Chula Vista	-0.036	0.067	0.588
Denver	-0.053	0.069	0.442
Detroit	-0.055	0.058	0.343
Harlem	-0.037	0.062	0.554
Inglewood	-0.044	0.060	0.462
Jacksonville	-0.030	0.061	0.623
Lexington	-0.028	0.061	0.646
Minneapolis	-0.015	0.061	0.811
Philadelphia	-0.037	0.060	0.532
Portland	-0.056	0.062	0.362
Salem	-0.046	0.069	0.507
San Jose	-0.023	0.066	0.722
Age 20-22	-0.002	0.039	0.958
More than 1 child	-0.053 *	0.030	0.079
Age 16 or younger when first child was born	0.014	0.026	0.598
Ever had an abortion	-0.010	0.030	0.732
Has a driver's license	0.010	0.028	0.726
Ever had a miscarriage	0.020	0.033	0.548
Has no home phone	-0.022	0.029	0.462
Hispanic	0.030	0.035	0.396
Not black or Hispanic	0.040	0.034	0.233
Highest grade completed is above 10th	0.003	0.025	0.897
Has a high school diploma or GED	0.028	0.053	0.596
Reading level (grade equivalent) is below 8th grade[c]	0.031	0.022	0.164
Ever had vocational/occupational skills training	0.004	0.026	0.891
Highest educational goal is high school/GED or other	0.026	0.023	0.257
Receives child support from first child's father	0.016	0.025	0.518
Did not use birth control when last had sex	0.002	0.024	0.940
Has regular child care[d]	0.029	0.023	0.200
Was pregnant more than twice	0.079 **	0.035	0.024
Youngest child is older than 1 year old	-0.019	0.024	0.430
Locus of Control score is less than 21[e]	0.014	0.024	0.541
Self-esteem score is less than 35[f]	0.018	0.026	0.490
Ever repeated a grade	-0.016	0.023	0.494
Not receiving AFDC in own name	0.018	0.036	0.622
Heard about New Chance from welfare officer	-0.025	0.027	0.361
Not JOBS-mandatory	0.015	0.040	0.712
At risk of depression	-0.044 *	0.023	0.053
Currently or previously married	-0.050	0.038	0.187

(continued)

Table A.2 (continued)

Variable[a]	Parameter Estimate	Standard Error	p[b]
Family never on welfare when sample member growing up	0.017	0.023	0.463
Living in public housing	-0.032	0.027	0.240
Received family planning services in the prior 60 days	0.038	0.026	0.139
First child's father never sees child	0.003	0.023	0.889
Does not expect to have more children	-0.004	0.023	0.875
Lived with father at age 14	-0.009	0.025	0.727
Ever employed in prior 12 months	-0.029	0.031	0.349
Earned $501 or more in prior 12 months	0.035	0.035	0.314
Never employed	-0.008	0.029	0.782
Sample size	2,079		
Number of experimentals	1,401		
Number of controls	678		
Mean of dependent variable	0.677		
R-square	0.016		
F-statistic	0.661		
P-value of F-statistic	0.970		

SOURCE: MDRC calculations from the New Chance Enrollment Form data.

NOTES: The dependent variable in each regression equation was unity for each experimental and zero for each control. Each characteristic on the right-hand side of each equation was measured as a deviation from its mean.

The p-value of the F-statistic is the probability of obtaining these coefficient estimates if the true chance of becoming an experimental did not vary with any characteristic. Thus, the closer the p-value is to unity, the more successful was random assignment in equating average characteristics of experimentals and controls.

[a] No dummy variable for Pittsburgh was included, as this would overdetermine the regression model.

[b] A two-tailed t-test was applied to each coefficient estimate. The column labeled "p" indicates the statistical significance level of the coefficient: That is, p is the probability that the actual value coefficient is zero. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[c] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[d] Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

[e] The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

[f] The measure of self-esteem used was the Rosenberg self-esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

Appendix B

Sample Definition and Analysis of Survey Nonresponse

This appendix describes the samples on which the findings presented in this report are based and compares the characteristics of these samples with the entire sample of those who were randomly assigned. As is discussed in Chapter 2, the primary difference between those who were randomly assigned and those featured in this report is that none of the findings presented in this report include sample members who applied for New Chance and were randomly assigned but for whom no usable follow-up data were available. These sample members were treated as survey nonrespondents and excluded from the impact analyses. This appendix explores the extent to which the survey respondent samples are representative of the full New Chance study sample. In summary, it shows that the response rates on both the 18-month and the 42-month follow-up surveys were quite high, resulting in a study sample.that is a good representation of randomly assigned young women. Differences between the full sample and the respondent subsample are small and often not statistically significant. More importantly, the extent of *differential* nonresponse was found to be very small for all of the respondent samples used in this report; that is, experimentals and controls were generally found to be equally likely to respond to either one of the follow-up surveys, to answer specific questions on each one of those surveys, and to cooperate with the teacher survey effort.[1]

Table B.1 shows survey response rates by research status and site. The overall response rate was approximately 90 percent in both surveys. This response rate is generally considered very high in a population such as this one. Experimentals were somewhat more likely to respond to the follow-up surveys than controls, and there was some variation in response rates across the sites. In the first follow-up survey, Harlem had the lowest response rate (82.7 percent) and the response rate was highest in Detroit, where outcome data were collected for 96.6 percent of the sample members. In the second follow-up survey, Minneapolis had the highest response rate (97.6 percent) and Chicago the lowest (85.9 percent).

For most, but not all, of the analyses presented in this report, a common respondent sample of 2,079 was used. This sample excludes 26 sample members who, while being respondents, provided invalid answers to important questions in key sections of the 42-month follow-up survey. For some sections of the report, additional sample members were dropped because of missing or invalid data, or because the sample members did not respond to the 18-month survey. For other sections, sample members were excluded because no child data or teacher data were available for them. Finally, individual analyses sometimes excluded some sample members who did not have valid data for particular outcomes.

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[1] At the time of the 42-month survey, mothers with school-age children were asked to give researchers permission to contact the teachers of these children. They were also asked for information used to seek contact with the teachers. There were no experimental/control differences in the degree of cooperation with this effort to survey the teachers.

Table B.1

Survey Response Rates, by Research Group and Site

Research Group and Site	Sample Size	18-Month Survey Response Rate (%)		p^a	42-Month Survey Response Rate (%)		p^a
Full sample	2,322	89.9			90.6		
Research group			*	0.092			0.103
Experimental	1,553	90.7			91.3		
Control	769	88.4			89.2		
Site			***	0.000		*	0.065
Allentown	124	92.7			91.9		
Bronx	150	84.0			90.7		
Chicago Heights	78	88.5			85.9		
Chula Vista	150	84.7			88.0		
Denver	123	89.4			87.8		
Detroit	175	96.6			94.3		
Harlem	150	82.7			86.0		
Inglewood	146	89.7			90.4		
Jacksonville	154	93.5			92.2		
Lexington	150	90.0			93.3		
Minneapolis	127	95.3			97.6		
Philadelphia	150	90.0			91.3		
Pittsburgh	180	95.0			90.0		
Portland	166	86.1			87.3		
Salem	150	89.3			88.7		
San Jose	150	89.3			92.7		

SOURCE: MDRC calculations from New Chance Enrollment Form and survey data.

NOTE: [a]A t-test or F-test was applied to the differences in response rates among research groups and sites. The column labeled "p" is the statistical significance level of this difference: That is, p is the probability that this difference exists only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table B.2 compares baseline characteristics for five key samples. The first is the full randomly assigned research sample of 2,322, which is the natural reference point for all analyses of nonresponse. The second column describes the 42-month survey respondent sample of 2,079, the primary sample in this report. The third column looks at 42-month respondents who also responded to the 18-month survey (1,939). This subsample is used for analyses that use point-in-time data from the 18-month survey.[2] The final two columns focus on the sample of "focal" children for whom special survey data and teacher data were collected. The child outcomes sample, featured in the fourth column, includes all focal children for whom survey modules were completed, while the teacher survey sample (last column) includes only school-age children whose teachers completed a mail-in survey. The asterisks next to each column indicate whether the differences in demographic characteristics between the featured respondent sample and the full sample were found to be statistically significant. As expected, most statistically significant differences appear for the teacher survey sample, whose composition is most different from that of the other samples because of restrictions on the age of the focal child.

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[2] Note that the 42-month survey gathered a great deal of continuous panel data for the early part of the follow-up period for nonrespondents to the 18-month survey.

Table B.2

Selected Characteristics of the New Chance Sample at Random Assignment, by Survey Respondent Subsample

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Experimental status					
Experimental	66.9	67.4	67.6	66.9	66.3
Control	33.1	32.6	32.4 *	33.1	33.7
Demographic characteristics					
Age (years) (%)				**	***
16	2.1	1.9	1.9	1.9	1.0
17	17.3	17.4	17.7	17.3	11.3
18	21.9	22.2	22.3	23.5	21.1
19	25.6	25.8	25.8	25.8	23.6
20	20.0	19.6	19.1	19.3	24.0
21	11.1	11.0	11.1	10.5	16.2
22	1.9	2.0	2.1	1.8	3.0
Average age (years)	18.9	18.8	18.8	18.8 **	19.2 ***
Ethnicity (%)		*	*	***	***
Black, non-Hispanic	52.2	52.4	52.4	54.3	56.5
Hispanic	22.3	22.8	23.1	23.2	23.1
White	22.9	22.5	22.2	20.3	17.8
Other	2.5	2.3	2.3	2.2	2.5
Marital status (%)					
Never married	90.1	90.2	90.2	91.3 ***	89.3
Other	9.9	9.8	9.8	8.7	10.7
Number of children (%)					***
1	65.0	65.0	65.3	66.0	58.6
2	26.8	26.7	26.3	26.3	30.3
3 or more	8.2	8.2	8.4	7.7	11.2
Average number of children	1.4	1.5	1.4	1.4 **	1.5 ***
Age of youngest child (years) (%)					***
Less than 1	53.5	53.3	53.5	53.5	36.9
1	27.1	27.4	27.0	27.1	30.3
2	11.7	11.5	11.8	11.7	18.8
3 or older	7.7	7.8	7.7	7.7	14.0

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Average age of youngest child (years)	1.2	1.2	1.2	1.2	1.6 ***
Age at first child's birth (years) (%)					***
14 or under	5.3	5.2	5.2	5.1	6.0
15	12.1	12.1	12.1	12.3	14.7
16	23.0	23.2	23.1	23.1	26.8
17	26.3	26.3	26.3	26.1	26.2
18	21.1	21.2	21.6	21.4	17.2
19	12.3	12.0	11.7	12.0	9.0
Average age at first child's birth (years)	16.8	16.8	16.8	16.8	16.6 ***
Living arrangement					
Living with (%)					
Mother	33.9	34.3	34.6	35.6 ***	30.5 ***
Father	7.9	8.3 **	8.1	8.5 **	6.7
Spouse or partner	11.5	11.0 *	11.2	10.2 ***	9.7 **
No other adult	32.2	32.4	31.9	31.5	38.7 ***
Lived in a female-headed household at age 14 (%)	48.9	48.3 *	48.3 **	49.0	49.1
Lived with both parents at age 14 (%)	22.6	23.0	22.8	23.8 **	23.4
Education characteristics					
Highest grade completed (%)		*		**	**
7th or below	3.1	2.7	2.7	2.4	2.5
8th	10.6	10.7	10.3	10.5	9.1
9th	22.9	23.0	22.8	22.5	20.0
10th	30.6	30.7	30.5	30.7	33.0
11th	27.4	27.6	27.9	28.3	29.2
12th	5.4	5.3	5.6	5.7	6.1
Average highest grade completed	9.8	9.9	9.9 **	9.9 ***	9.9 ***
Had high school diploma or GED (%)	5.9	6.1	6.4 **	6.2	6.4
Left school before first pregnancy (%)	38.3	37.6 **	36.9 ***	37.2 **	30.2 ***

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Average number of years since last attended school	2.4	2.4 **	2.4 ***	2.3 ***	2.6 ***
Reading level[a] (grade equivalent) (%)				*	
4th grade or below	9.0	8.8	8.6	8.4	8.4
5th grade	5.9	5.6	5.7	5.6	5.6
6th grade	9.2	9.1	9.2	9.4	10.7
7th grade	10.8	10.7	10.7	10.8	10.6
8th grade	14.0	14.5	14.5	14.7	14.3
9th grade	20.9	21.2	21.4	21.7	21.6
10th grade or above	30.2	30.0	29.8	29.3	28.6
Average reading level[a] (grade equivalent)	7.6	7.6	7.6	7.6	7.6
Desired educational attainment for self (%)					
High school diploma or GED	33.1	33.3	32.7	33.9	33.5
1-3 years of college (A.A. degree)	30.6	31.0	31.5	30.9	29.2
4 years of college (B.A. degree)	22.1	21.4	21.6	21.1	22.0
Graduate degree	10.8	10.9	10.8	10.6	12.3
Other	3.3	3.4	3.4	3.5	3.0
Desired educational attainment for child[b] (%)					
Elementary school	0.2	0.2	0.2	0.2 *	0.2
High school	21.3	21.7	21.2	22.4	23.3
College/post-secondary	57.3	57.0	57.5	56.8	55.9
Graduate school	21.3	21.1	21.1	20.6	20.5
Mother has high school diploma or GED (%)	52.2	52.3	52.2	51.9	49.6
Mother attended college (%)	25.5	25.5	25.0	24.7	24.8
Father has high school diploma or GED (%)	42.6	42.8	43.0	42.8	41.1
Father attended college (%)	16.0	16.2	16.2	15.5	14.3
Both parents have high school diplomas or GEDs (%)	28.9	28.9	29.1	28.4	26.8
Both parents attended college (%)	7.6	7.7	7.3 *	7.0	6.6

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Employment and welfare receipt					
Number of jobs ever held (%)		**			
0	22.0	21.2	21.4	21.5	21.5
1-2	33.1	33.4	33.3	33.8	33.0
3 or more	44.9	45.5	45.4	44.7	45.5
Average number of jobs held	2.6	2.6	2.6	2.6 *	2.6
Employed at random assignment (%)	3.1	3.0	3.1	3.1	3.6
Number of months employed in prior 12 months (%)					
0	63.6	63.3	63.3	63.3	64.0
3 or less	18.7	18.8	18.6	18.8	18.7
4-6	10.0	10.1	10.1	9.9	9.1
7-12	7.6	7.8	8.0	8.0	8.2
Prior-year earnings (%)					
$0-$500	80.2	80.2	80.0	80.2	79.2
$501 or more	19.8	19.8	20.0	19.8	20.8
Length of longest job (%)		**	**	**	
Never employed	21.5	20.6	20.7	20.9	20.9
Less than 1 month	4.0	4.0	3.6	3.6	3.8
1-3 months	22.7	23.0	23.0	23.5	23.2
4-6 months	22.3	22.8	22.7	22.5	22.0
7-12 months	17.6	17.8	18.0	18.1	16.9
Over 1 year	11.9	11.8	12.0	11.4	13.2
Mother employed (%)					
Yes	49.3	49.2	49.6	49.3	49.7
No	42.1	42.5	42.3	42.5	41.4
Don't know	4.4	4.2	4.1	3.9	4.8
Deceased	4.2	4.2	4.0	4.3	4.1

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Father employed (%)					
Yes	45.1	45.4	45.5	45.0	44.8
No	19.9	20.1	20.1	20.2	19.8
Don't know	25.2	24.8	24.5	24.6	24.5
Deceased	9.8	9.7	9.8	10.1	10.9
Receives AFDC (%)		***	***	**	***
Own grant	87.2	87.5	87.4	87.3	91.4
Other person's grant	7.2	7.4	7.6	7.7	4.4
Not receiving AFDC	5.6	5.1	5.0	5.0	4.2
Receives (%)					
Medicaid	87.3	87.7	87.3	87.7	89.0
Food stamps	83.8	83.8	83.6	83.4	86.5 **
Public housing	23.4	23.6	23.5	24.3	28.0 ***
Income from a job	3.5	3.2 **	3.3	3.3	4.0
Family received AFDC when sample member was growing up (%)					
Never	16.7	16.6	16.6	16.4	17.8
2 years or less[c]	18.4	18.4	18.8	18.3	18.0
More than 2 years, but not always[c]	28.3	28.7	28.8	29.2	28.3
Always	36.6	36.4	35.9	36.0	35.9
Fertility-related characteristics					
Number of pregnancies (%)					***
1	43.2	43.1	43.4	43.7	34.1
2	32.3	32.1	32.1	32.2	35.1
3	16.2	16.1	16.0	16.0	20.5
4	6.0	6.2	6.0	5.7	7.6
5 or more	2.3	2.5	2.5	2.4	2.8
Average number of pregnancies	1.9	1.9	1.9	1.9	2.1 ***
Ever had an abortion (%)	23.1	23.4	23.3	24.0 **	28.0 ***

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
When next child is expected (%)					
Not expecting another child	64.5	64.3	64.0	63.5	63.5
Within 2 years	1.7	1.7	1.7	1.6	2.3
In 2-4 years	16.8	16.8	17.0	16.7	18.0
In 5 years or more	17.1	17.2	17.3	18.1	16.2
Average number of years until next child is expected[d]	4.4	4.4	4.4	4.5	4.4
Current birth control use (%)				*	*
Yes, using birth control	61.5	61.4	62.0	62.3	61.1
No, not using birth control	12.4	12.4	12.1	11.5	14.5
No partner/not having sex	26.1	26.2	25.9	26.2	24.4
Used birth control at last intercourse (%)	70.5	70.2	70.7	71.0	71.4
Relations with child's father					
Speaks with child's father[b] (%)	66.6	67.3 **	67.8 ***	67.4	64.5
Has child support order[b] (%)	27.2	27.7	28.2 **	27.8	28.9
Prior and current service receipt					
Ever in occupational skills training (%)	22.4	22.6	22.6	21.9	24.5 *
Services received in the 60 days before random assignment (%)					
Health care for child[b]	84.5	84.8	84.9	85.3 **	82.4 **
Family planning	23.2	23.5	23.0	23.7	20.5 **
Mental health	2.8	2.8	2.8	2.5 **	3.0
Health care for self	59.0	58.9	58.6	59.2	55.2 ***
Parenting	11.3	11.0	10.9	10.9	9.9
Life skills	3.1	3.1	3.0	2.7 **	2.2 *
Counseling	4.1	4.0	4.1	3.6 **	2.7 **
Other services	10.3	10.6	10.8 *	10.8	8.0 ***
No services	8.3	8.3	8.4	8.5	10.7 ***
Has regular child care[e] (%)	44.2	44.6	44.0	44.5	43.2

(continued)

Table B.2 (continued)

Characteristic and Subgroup at Random Assignment	Full Sample	42-Month Survey Respondent Sample	18-Month Survey Respondent Sample	Child Outcomes Sample	Teacher Survey Respondent Sample
Psychosocial characteristics					
CES-D (depression) Scale[f] score (%)					
0-15 (not at risk)	46.5	46.6	46.6	47.3	47.5
16-23 (at some risk)	25.7	25.3	25.6	25.2	25.5
24-60 (at high risk)	27.8	28.1	27.9	27.6	27.0
Average CES-D score[f]	18.1	18.1	18.2	18.0	17.9
Average number of sources of emotional support	2.8	2.8	2.8	2.8	2.6 **
Average level of satisfaction with emotional support[g]	4.2	4.2	4.2	4.2	4.2
Average self-esteem score[h]	38.3	38.4	38.3	38.5 **	38.6
Average Locus of Control score[i]	22.0	22.0	22.0	22.0	22.1
Other					
Has home telephone (%)	83.7	83.6	83.8	84.7 **	82.5
Has driver's license (%)	27.3	27.5	27.9	26.3 **	27.7
Sample size	2,322	2,079	1,939	1,807	835

SOURCES: MDRC calculations from New Chance Enrollment Form data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The actual sample sizes for individual measures presented in this table may fall short of the reported sample sizes because of missing or unusable items from some sample members' questionnaires.

Distributions may not total 100.0 percent because of rounding.

[a] The test used to measure reading ability was the reading part of the Tests of Adult Basic Education (TABE). Most sites administered the Survey Form of the test, but some administered the full reading test.

[b] When a sample member had more than one child, her response refers to her first child.

[c] The family's AFDC receipt may not have been continuous.

[d] Includes only those sample members who expected to have more children.

[e] Regular child care was defined as an ongoing arrangement used while the mother was in school, in training, or working.

[f] The Center for Epidemiological Studies Depression (CES-D) Scale is a widely used measure of depression; scores can range from zero to 60.

[g] Enrollees were also asked about their degree of satisfaction with the emotional support ("people who listen to you, reassure you, and show you they care") they received. Levels range from 1 (very dissatisfied) to 5 (very satisfied).

[h] The measure of self-esteem used was the Rosenberg Self-Esteem Scale, a 10-item scale that assesses a person's global sense of self-worth. Scores can range from 10 to 50; 30 is considered the neutral midpoint.

[i] The Locus of Control Scale is a six-item adaptation of the longer scale originally developed by Julien Rotter (1966). Scores can range from 6 to 30; 18 is considered the neutral midpoint.

Appendix C

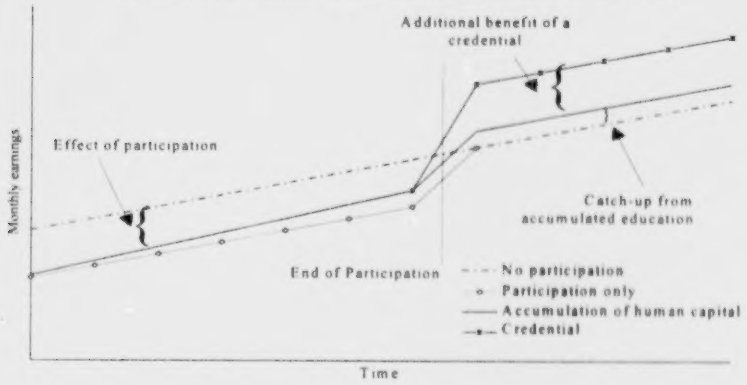
Estimating the Effects of Basic Education, Skills Training, and Education Credentials on Monthly Earnings

This appendix discusses the analytical methods used to produce the estimates presented in Tables 7.9 and 7.10 and accompanying sections of Chapter 7. The purpose of this analysis is to measure the effects of participation in adult education (ABE/GED), skills training, and college on monthly earnings, as well as the effects of education credentials associated with these services. The analysis produces separate estimates of the opportunity cost of current participation in these services and the longer-term payoff from accumulation of adult education, skills training, and college. As is discussed in Chapter 7, these estimates contribute to our understanding of the dynamics behind the New Chance program and its impact results.

Analytical Methods

This analysis is based primarily on variation in education and employment outcomes over time. The idea is (1) that participation in education or training has an opportunity cost associated with it, (2) that, over time, accumulation of education and training increases the value of participants' contribution to the labor market, and (3) that education credentials boost this human capital effect (see theoretical model below for a graphic representation of these hypothesized effects). By transforming the data from person data into person-month data it becomes possible to use variation in outcomes over time to estimate regression coefficients for each of these effects. That is, for the average person, earnings and other employment outcomes are expected to be lower during participation in education or training, to increase with accumulation of human capital, and to increase sharply following attainment of an education credential.

Hypothetical Effects of Remedial Education on Earnings



By including *fixed effects* it is possible to reduce drastically the threat of selection bias in an analysis like this one.[1] Such fixed effects remove the person-specific component from the outcome variable, leaving variation over time as the only source of inference. In this analysis, fixed effects are controlled for by differencing the person-time observations of outcomes and explanatory variables with person-specific means. Thus, for instance, the analysis uses as its "outcome" the difference between earnings for person *i* in Month *j* and mean monthly earnings for person *i* across the 42 months of follow-up, as in the following model:

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[1] See Ashenfelter (1978), Kiefer (1979), and Ashenfelter and Card (1985) for some applications of fixed effects models.

$$Y_{ij} - \overline{Y}_{i\bullet} = \beta_j(mth_j - \overline{mth_j}) + \sum_{1}^{k} \beta_{X_k ij}(X_{kij} - \overline{X}_{ki\bullet}) + \varepsilon$$

in which the outcome variable $Y_{ij} - \overline{Y}_{i\bullet}$ is the deviation of monthly earnings from its mean over 42 months, the term $mth_j - mth_j$ represents a series of time dummies included to capture the trend in monthly earnings over time, and the term $X_{kij} - X_{ki\bullet}$ represents the deviations of a series of education variables X_k from their mean over 42 months. As shown in the table below, the variables X_k include variables that measure current participation in education, skills training, and college (thus capturing the opportunity cost associated with participation), ones that measure accumulated education and training, and ones that capture education credential effects that are independent of the effects estimated with the participation variables.

Analysis Variables X_k

- Current participation
  - Weeks in ABE/GED
  - Weeks in skills training
  - Weeks in college
- Accumulation of human capital
  - Total weeks in ABE/GED
  - Total weeks in skills training
  - Total weeks in college
- Education credentials
  - GED certificate
  - High school diploma
  - Trade license/training cert.
  - College credit

An important potential problem in the analysis of time-series data like these is *autocorrelation*. This statistical phenomenon (stemming from the tendency of things to be related over time) affects the efficiency of the estimates and deflates their standard errors. As a result, estimates that appear to be statistically significant may not be. To address this problem, the analysis is performed with a second-order autoregressive model (PROC AUTOREG in SAS).[2] Consequently, the resulting estimates are free from autocorrelation.

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[2] An *autoregressive* estimation procedure attempts to eliminate the effects of autocorrelation by explicitly estimating the time parameters underlying the autocorrelated errors in the regression. In doing so, it not only removes the self-explaining part of the error term (leaving only random noise), it also improves the precision and explained variance of the model as a whole. As a result, the standard errors of the estimates are no longer biased. The information contained in the newly estimated time parameters is also used to improve the estimates of other coefficients in the model. In schematic form, this procedure works as follows:

$$Y_j = \beta_j + \beta_{mj} X_{mj} + v_j$$

is an equation with an autocorrelated error term v_j. Assuming a first-order autocorrelated error structure (AR1), the relationship between individual error terms v_j may be written as follows:

$$v_j = \beta_j x v_{j-1} + \varepsilon_j$$

where β_j is the autoregressive parameter. This parameter describes the correlation of errors v_j over time, allowing ε_j to be a truly independent random error term. As the autoregressive parameter measures relationships over time in the data, it also may explain variation in the previous equation that could otherwise be falsely attributed to explanatory variables X_{mj} in the model. The Yule-Walker method, which is the preferred method for estimation of autoregressive models with SAS, uses the residuals from a simple OLS estimation of the first equation to estimate the autoregressive parameter β_j. This information is then used to generate an unbiased estimate of the variance in the model and to compute adjusted parameters β_{mj}. Both steps involve GLS estimation methods.

Caveats

While fixed-effects models using panel data offer substantial protection against selection bias, there is a possibility that variation in the *slopes* of earnings and employment profiles may affect the analysis.[3] It is very difficult to control for this type of bias, because to do so requires estimating these slopes on continuous pre-program outcome data, which is unavailable for the New Chance sample (no administrative data were collected). Also, there is a slight possibility that estimates like these produced with fixed-effects models are subject to history bias.[4] It is difficult, however, to conceive of unmeasured events that might consistently co-occur with the education variables used in the analysis.

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[3] This type of selection bias is also referred to as an inherent difference in the "potential for change."

[4] History bias occurs when changes in an outcome are attributed to changes in one variable, while they are really the result of changes in another, not included in the analysis. For instance, if GED attainment in New Chance had consistently been accompanied by intensive co-occurring job placement efforts, the regression coefficient associated with the GED would have captured the combined effect of GED attainment and job development. In that case, the GED coefficient would no longer provide a valid estimate of the effect of a GED on earnings.

Appendix D

Table D.1

Impacts of New Chance on Receipt of a High School Diploma or GED Certificate Within 42 Months After Random Assignment, by Site

Site	Sample Size	Experimentals (%)	Controls (%)	Within-Site Impact	p[a]	Difference Across Site Impacts p[a]
						0.059 *
Allentown	114	61.9	44.8	17.2 **	0.047	
Bronx	133	42.8	25.3	17.5 **	0.029	
Chicago Heights	63	46.9	39.2	7.7	0.516	
Chula Vista	131	50.3	50.9	-.6	0.944	
Denver	106	60.3	60.1	.1	0.990	
Detroit	161	41.3	51.0	-9.7	0.181	
Harlem	126	50.9	42.7	8.3	0.319	
Inglewood	130	58.8	33.1	25.7 ***[b]	0.001	
Jacksonville	142	45.3	29.7	15.6 **	0.046	
Lexington	136	41.5	40.8	.7	0.934	
Minneapolis	124	69.7	65.8	3.9	0.641	
Philadelphia	137	36.3	38.0	-1.8	0.826	
Pittsburgh	160	73.6	58.3	15.4 **	0.039	
Portland	145	65.3	62.3	3.0	0.696	
Salem	132	37.7	35.8	1.9	0.817	
San Jose	139	67.0	43.6	23.4 ***[b]	0.003	
Sample size	2,079					

SOURCES: MDRC calculations from New Chance Enrollment Form and survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

The percentages are adjusted using a two-way analysis of covariance procedure controlling for up to 36 kinds of difference in characteristics, other than site, before random assignment. The two categories used as factors were research status (i.e., membership in the experimental or control group) and site. Rounding may cause slight discrepancies in sums and differences.

[a] A two-tailed t-test was applied to each regression-adjusted within-site impact. An F-test was applied to the interaction between sites and experimental or control status. The columns showing p-values are the statistical significance levels of each within-site impact and the difference in impacts across sites: That is, p is the probability that sample estimates are different from zero or from each other only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

[b] Impacts at these sites remained statistically significant after interactions between research status and the 36 non-site characteristics were added to the procedure.

Appendix E

Methods to Identify Determinants of Child Behavior Problems

This appendix reports on the statistical methods used to analyze the determinants of child behavior problems reported in Chapter 8. These analyses were primarily done with SYSLIN, an SAS procedure that performs two-stage least squares analysis.

Analytical Framework

The analyses presented here were limited to a single outcome variable, the full Behavior Problem Index (BPI) as reported by the mother at 42 months after random assignment. This variable was standardized using nationally normed tables, which are centered around a score of 100, with a standard deviation of 15.

To explore the relationship between day care and other explanatory variables (**x**) and the outcome variable *(Y)*, the following regression model is our point of departure:

$$Y = b_0 + b_x \mathbf{x} + \sum_1^k b_k \mathbf{z} + \varepsilon$$

In this model, Y is the BPI score, **x** is a vector of explanatory variables (including day care, maternal depression, and residential mobility), and the vector **z** contains the baseline covariates. Unfortunately, however, the variation in **x** is not created with random assignment, and this model is therefore vulnerable to possible misspecification bias. If both **x** and Y were correlated with an unmeasured variable Q, this variable could offer an alternative explanation for the alleged relationship between **x** and Y, thus biasing the coefficients b_x in its absence.

Fortunately, there is a very strong relationship between E, the program variable identifying assignment to New Chance, and the child care variables in **x**. Assuming that program effects on child outcomes are mediated by program effects on child care, we can use the fact that E is uncorrelated with individual characteristics (measured or unmeasured) to remove selection bias from the relationship between **x** and Y. This is done by using two-stage least-squares to replace variables X in **x** in the above equation with variables X^*, which instead of the actual values of X contain predicted values from a regression of X on E and other exogenous baseline variables, as follows:

$$X = b_0 + b_1 E + b_k \mathbf{z} + \varepsilon$$

$$X^* = \hat{X}$$

$$Y = b_0 + b_1^* X^* + b_k \mathbf{z} + \varepsilon$$

The coefficient b_1* captures the relationship between X and Y without potential selection bias from unmeasured variables Q.

An important assumption underlying this technique is that the entire experimental effect on the BPI outcome is mediated by the experimental effect on the mediating variables in **x**. Thus, in an analysis involving a child care variable X_c, the residuals of a regression of BPI scores (Y) on the child care variable (X) must be uncorrelated with the experimental dummy (E). Otherwise, the estimated effect of the instrumented child care variable on the BPI score might reflect some of this direct correlation between E and Y. For example, if the experiment, in addition to affecting BPI scores by increasing day care use among very young children, also affected BPI scores by increasing depression among sample members, the coefficient on the instrumented day care variable would capture both the effect of the increase in day care and the effect of the increase in depression. Unfortunately, this potential bias can not be ruled out definitively, and the direction of the bias caused by "unexplained" effects of E on Y is not obvious. While an experimental effect on depression would probably *inflate* the estimated effect of child care on BPI scores, an experimental effect on participation in parenting classes would probably *deflate* it.

It is possible to address this concern by including in the first and second stages of the analysis a vector **v** containing all mediating variables in **x**, except for the explanatory (child care) variable X_c, as follows:

$$X_c = b_0 + b_1 E + b_m \mathbf{v} + b_k \mathbf{z} + \varepsilon$$

$$X_c^* = \hat{X}_c$$

$$Y = b_0 + b_c^* X_c^* + b_m \mathbf{v} + b_k \mathbf{z} + \varepsilon$$

While it would be difficult to identify all possible variables X_m in **v**, it is possible to identify some key 18-month outcomes that are both strongly affected by E and strongly related to Y. The analyses presented in Chapter 8 use variables that measure participation in parenting classes, living arrangements, residential mobility, fertility outcomes, depression, HOME scores, and GED attainment. Except for the residential mobility variable, all variables were measured before the 42-month interview to rule out ambiguity about the causal direction between Y and **v**.

Appendix F

New Chance Site Profiles

Expectant and Parenting Youth Program, Private Industry Council of Lehigh Valley Allentown, Pennsylvania

As the local Job Training Partnership Act (JTPA) operator, the Private Industry Council (PIC) of Lehigh Valley has provided occupational skills training programs to youth and adults in the Allentown/Easton/Bethlehem area since 1983. New Chance operated as part of the PIC's Expectant and Parenting Youth Program (EPYP), which was created in 1985 to serve 14- to 21-year-old pregnant and parenting women who are high school dropouts. Housed at the PIC's offices in a predominantly residential working-class area of Allentown, EPYP/New Chance used PIC services, including on-site vocational training programs, an IBM PALS (Principles of Alphabet Literacy System) computer learning center, on-site child care, work internship development, and job placement assistance. The program served an ethnically mixed group of black, Hispanic, and white young mothers. It tended to attract young women residing in Allentown, but participants also came to the program by bus, private transportation, or the agency's van from other cities in the Lehigh Valley.

EPYP offered many of the New Chance activities before joining the demonstration and was nationally recognized as a strong provider of education and parenting services for adolescent parents. EPYP is approved by the state's Department of Education as an alternative school for pregnant and parenting teens. The on-site day care center is an integral part of the parenting component and offers daily opportunities for staff to work with the young women and their children. The day care staff are employees of EPYP and coordinate child care services and parenting instruction with other activities.

During the period under study, EPYP's staff expanded the program's focus on employment-related services by enhancing the career exploration and pre-employment skills activities, making work internships a year-round option, and connecting participants with vocational training programs. The program also added family planning classes. In addition, the program formed linkages with other agencies to provide services, including workshops offered by instructors from the Penn State Cooperative Extension and from Planned Parenthood.

EPYP/New Chance staff incorporated some services that made the site's program especially comprehensive and responsive to the teens' needs. Most notable were the on-site clinic held twice a month by the Visiting Nurse Association (VNA) and the group home operated by EPYP. The monthly VNA clinics provided a highly accessible setting for New Chance mothers and their children in which medical staff could treat acute problems, provide immunizations and well-care checkups, and highlight the importance of regular health care. The EPYP/New Chance group home, which can accommodate up to five families at one time, enabled the program to respond to housing emergencies and helped prepare young women for independent living. A resident Life Skills Coordinator reinforced in the home setting what the teens learned in their life skills, nutrition, and health classes.

The program's education component was refined and restructured over the course of the demonstration to allow EPYP's strong team of teachers to deliver a more individualized education program responsive to the diverse needs of the students. In addition to creating separate sec tions for GED preparation and basic skills remediation, staff brought in volunteer tutors and developed a literacy lab.

As the first demonstration participants neared readiness for skills training (usually defined as passing the GED exam and having completed all Phase I activities), an employment specialist was hired to coordinate the referrals to skills training programs, either at the PIC or at other area training providers. When participants were ready for skills training, the employment specialist took on case management responsibilities for them on employment-related matters; personal issues continued to be handled by the participants' original case manager. For participants interested in and thought ready to sustain longer-term participation in associate's degree training courses, the program emphasized assistance in enrolling at one of the area's community colleges.

The employment specialist also assumed responsibility for teaching the program's employability development courses and developing and monitoring work internships. Job-shadowing opportunities and internships developed by the specialist were a key part of the program's strategy for enhancing participants' ability to make career choices and reinforce instruction in the employability development class. The PIC's Summer Youth Service Corps provided a vehicle for EPYP/New Chance participants interested in working in nontraditional fields.

Despite a well-planned recruitment campaign and strong linkages to two county welfare departments, recruitment was a challenge. Information from local welfare departments on the number of potential New Chance eligibles in the area indicated that the program was recruiting from a relatively small pool, but staff successfully drew in a large percentage of the eligible population.

Since the period under study in this report, the EPYP/New Chance program has begun emphasizing helping participants make the transition into skills training courses and has been successful in increasing the percentage of program completers who enter vocational training. Other post-demonstration enhancements designed to increase attendance and retention include expanded orientation to the program, recommitment contracts for participants with poor attendance, quarterly progress reports for all participants, and the county welfare staff's more active role in goal planning and progress review sessions for program participants.

The EPYP/New Chance program was fully supported by funds from the Single Point of Contact (SPOC) Program (part of Pennsylvania's JOBS program). All New Chance participants at this site had to be eligible for and enrolled in SPOC. The teen parents in New Chance were considered voluntary JOBS participants in Lehigh and Northampton counties. SPOC has continued to provide funding for the programmatic expansions that were made for EPYP's participation in the New Chance demonstration, allowing these changes to be institutionalized.

National Puerto Rican Forum, Inc.
Bronx, New York

The New Chance program operated by the National Puerto Rican Forum, Inc. (NPRF) was located in the South Bronx, a community with a national reputation for high rates of school dropout, youth unemployment, teen pregnancy, infant mortality, and drug-related injury and death. This Bronx agency, the flagship of a national network of community-based social service and advocacy organizations, had provided education, job skills training, and job placement services in this largely Latino neighborhood since 1978, using a combination of city, state, and private funding. In the fall of 1989, MDRC, the New York State Department of Social Services (DSS), and NPRF contracted to implement New Chance. The program operated as part of the Comprehensive Employment Opportunity Support Centers (CEOSC), a DSS initiative that served AFDC recipients with children under age 6. Because of a loss of CEOSC funding, NPRF ceased operating its New Chance program in September 1993.

NPRF's on-site education (featuring computer-assisted instruction), clerical skills training, job-readiness instruction, and job placement services were strong before New Chance was implemented. New Chance required NPRF to strengthen services for the parenting and health components, however, and to add career exploration and family planning activities as well as access to other skills training options. The modifications were made under the stewardship of NPRF's core New Chance staff—a project director, two case managers, and a parenting instructor.

Strong ties with community agencies helped this site to implement fully the New Chance model. While NPRF could provide child care on a temporary or emergency basis, it had to rely on nearby child care centers, family day care, and other providers to meet the child care needs of participants. Through Montefiore Hospital's Community Clinic and Bronx Lebanon Hospital, New Chance participants and their children received health care screenings and follow-up referrals for treatment. Staff from the municipal health department and Montefiore made classroom presentations on family planning, lead poisoning, and prevention of childhood injuries. In addition, state, city, and private agencies served as referral centers for child and family welfare issues. Finally, New Chance participants attended special conferences on career and education opportunities held at local colleges. Along with these agency ties, the site also built relationships with prospective employers in the community.

Child care problems, unstable housing arrangements, and physical abuse affected program attendance and retention, and GED attainment rates were lower than expected. Staff explored a variety of strategies to overcome these challenges, including home visits, increased personal counseling, and the use of participants who had been in the program for a while as mentors for new participants.

DSS and the New York City Human Resources Administration (HRA), the city welfare agency, both provided valuable financial support and referrals of eligible applicants. Local JOBS funds paid for participants' support services and training-related expenses.

Aunt Martha's Youth Service Center, Inc. Chicago Heights, Illinois (currently located in Park Forest)

Aunt Martha's Youth Service Center, Inc., a 25-year-old community-based youth organization, offers comprehensive services to young people and their families living in Cook and Will counties, south of Chicago. Begun as a counseling center, Aunt Martha's has expanded to offer education, employment and health services, child care, legal assistance, youth development activities, and foster care services.

Aunt Martha's adopted the New Chance model in 1986, when it became one of six agencies selected to participate in the national pilot phase of the program. New Chance was built on the agency's existing parenting, family planning, and employment services. Some services, including education, were expanded for both the pilot phase and the demonstration. Child care was provided off-site through a network of day care centers and family day care homes.

The area south of Chicago served by Aunt Martha's includes both suburban and rural towns, some comprising ethnically homogeneous, predominantly middle-class communities, others characterized by a greater socioeconomic mix, including less affluent, working-class, and poor neighborhoods. During the demonstration period, the program was located in a town that had a high unemployment rate and a substantial black minority population; the town and its surrounding area have experienced an out-migration of manufacturing employers.

During the period under study, the extent of Aunt Martha's catchment area and its suburban and rural character required staff to develop a diversified recruitment strategy to draw young mothers to the program. Staff made presentations at community agencies, hospitals, churches, schools, and a local chamber of commerce. Meetings were held with local welfare office staff, the state's Department of Rehabilitation Services, and the Job Corps. Public service announcements appeared on local radio and cable television stations, advertisements were placed in community newspapers, and flyers were posted in social service offices and local businesses.

The core New Chance staff at this site, a full-time coordinator and case manager, were supplemented by the job developer for Aunt Martha's and by instructors from other programs within the employment and training unit at the agency and from other units. These staff taught the education, employability development, parenting, health, life skills, and family planning components.

Aunt Martha's New Chance program offered a very limited number of work internships, but several participants who attained GEDs and completed the program's Phase I activities enrolled in skills training classes at area providers. Few training courses were available for participants who had not passed the GED exam, and in general there were relatively few skills training providers within the area. The majority of participants who entered skills training classes enrolled in clerical and nurse's aide courses at the Advanced Technical Training Center, a major training provider for the area. The job developer for Aunt Martha's assisted participants in finding jobs.

The women enrolled in New Chance experienced several serious problems, such as unstable child care and housing arrangements and domestic violence, which prevented regular attendance at the program. Lack of transportation was another serious barrier to participation. Aunt Martha's attempted to address these problems through more intensive counseling and linkages with organizations that aid battered women and provide emergency housing. The development of an incentive program of monetary and non-monetary rewards for good attendance and program achievements was also part of the program's strategy to improve participants' attendance.

Aunt Martha's has developed a special linkage with Project Advance and Project Chance, the two JOBS programs operated by the Illinois Department of Public Aid (IDPA). Both projects served as referral sources for New Chance and also as sources of funding for child care and transportation for New Chance participants co-enrolled in either project. The New Chance program also received grants from Project Chance for education and training services for participants who qualified for co-enrollment in that project.

Despite state budget cuts in early 1991, IDPA renewed its commitment to fund New Chance. Aunt Martha's also obtained continued funding from other sources, including the local JTPA program—a provider of employment-related services and child care for JTPA-eligible participants—the Marshall Fields Foundation, and the state's Board of Education, all of which ensured the continuation of services at the site, although the site has not been able to replace special demonstration-related funds that ended at the conclusion of the demonstration's operational phase in December 1992.

Since the close of the demonstration period, the New Chance program has introduced a substance abuse prevention workshop to help address issues participants face in coping with drugs and alcohol abuse in their environments, particularly among their partners and other significant people in their lives. Development of new services has been constrained by a persistent funding gap, although the site has continued to work on replacing the demonstration-related funds that it received through 1992, particularly with the aim of expanding case management capacity for New Chance participants. To close the funding gap, the program has operated since 1993 with reduced staffing. Funding constraints experienced by the New Chance program and other factors prompted a decision at Aunt Martha's to integrate all participants in the agency's employment and training programs—male and female, parenting and nonparenting—into classes that follow the New Chance model.

Del Rey Center
Sweetwater Union High School District
Chula Vista, California

Sweetwater Union High School District's New Chance program was located at the Del Rey Center, which was formed from the merger of an alternative high school and an adult school in

1986. Adult education, occupational skills training, and counseling services are co-housed with a computer-assisted learning remediation center and Regional Occupational Center programs. (Regional Occupational Centers are funded by the state of California to provide education and occupational skills training programs, mainly to high school–age youth.) The Del Rey Center provides comprehensive education, occupational skills training, and child care services to in-school youth, high school graduates, and dropouts who are at risk of early pregnancy or who are already pregnant or parenting.

Two newly constructed buildings housed the New Chance classrooms, counseling and administrative offices, and an infant day care center. The Del Rey Center's Director of Vocational Education became the full-time director of the program. Part-time instructors were hired for adult education (ABE/GED) classes and for workshops in Life Skills and Opportunities (LSO) parenting, employability development, and health/family planning. Full-time staff were hired for the positions of case manager and clerk-typist. The majority of the New Chance participants attended occupational skills training classes at local community colleges; others were enrolled in on-site training programs or in JTPA programs. The Del Rey Center provided free breakfasts and lunches for New Chance participants through the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and other programs. In addition, the director gathered donations of goods (such as home furnishings) and services for New Chance for use as incentives for participants.

To facilitate implementation of the New Chance program, formal linkage agreements were made with several community service organizations. The primary agreement was with the San Diego County Greater Avenues for Independence (GAIN) Program. GAIN, a statewide program, is administered through the California Department of Social Services; with the passage of the Family Support Act in 1988, it became the state's federally mandated JOBS program. GAIN provides assessment, education, employability development, and vocational skills training to AFDC recipients and also provides funding for child care, transportation, and some ancillary expenses (for example, textbooks, equipment, and uniforms) while participants are in the program. In addition, child care services and Medicaid coverage are extended for a year after participants begin work and discontinue welfare receipt. San Diego County GAIN identified AFDC recipients who met New Chance's eligibility criteria and mailed them material provided by the New Chance program. Recipients who co-enrolled in New Chance and GAIN were eligible for GAIN services.

The site's notable recruitment success was due in large part to the support of the GAIN program, but it also reflects the time and effort staff invested in maintaining the interest of potential applicants who had to delay participation—sometimes for several months—until a new enrollment cycle began. Facing severe budget cuts in 1992, the Sweetwater Union High School District discontinued New Chance operations, and the San Diego County GAIN program continued to provide case management and services for young women enrolled in New Chance at that time.

Technical Education Center–North Campus
Community College of Denver
Denver, Colorado

The Technical Education Center (TEC) is a branch of the Community College of Denver located just north of the Denver city limits. TEC has offered adult education, occupational skills training, and job search and job placement services to disadvantaged men and women since it was founded in 1983. The programs and services at TEC are individualized, with an emphasis on computer-assisted instruction. TEC offers a range of occupational skills training options including courses that prepare students to be personal computer support specialists or human services paraprofessionals, and classes in accounting, information processing (including word processing), machine tool operating, and welding.

All New Chance services, except for health care and those given through Head Start, were provided on the TEC campus during the period under study. New Chance students enrolled in adult education (ABE/GED) classes with other TEC students. New Chance participants also attended Living Competencies, a one-semester course exclusively for them, which encompassed parenting and child development instruction, family planning and health education, employability development, and the LSO curriculum. This course is a strong example of the integration of several different New Chance subject areas, as emphasized in the program's guidelines. TEC has incorporated the Living Competencies course into all its core training options and has opened participation in it to any parent enrolled at TEC.

TEC was selected as a New Chance site because of its demonstrated success in helping disadvantaged people receive a GED and obtain occupational skills. Living Competencies was developed to include the health and personal development components of the New Chance model, as well as to strengthen the employability development component. The implementation of New Chance also led TEC to open an on-site developmental child care center in January 1990 that can accommodate 60 infants and toddlers. For children age 3 and older, New Chance used the Adams County Head Start program for day care, located near the campus.

The core New Chance staff at TEC includes a program manager/case manager, a full-time case manager, and a Living Competencies instructor. TEC staff teach the GED preparation and occupational skills training courses on campus, and guest speakers from local health clinics supplement the health education instruction.

The TEC New Chance program accepts residents from Adams and Denver counties. Before the program began, TEC already had a strong linkage for referrals with the Adams County Department of Human Development's JTPA program. Since the implementation of New Chance, the site has worked to develop a similar linkage with the Denver County Department of Social Services. Despite these efforts, recruitment has been the main challenge faced by TEC New Chance staff. Much of the eligible Adams County population is scattered in small towns throughout the county, and transportation is often difficult unless applicants have access to a car. In Denver County, transportation problems and competition from more established programs in the Denver metropolitan area have made recruitment of this population difficult. Among those enrolled in New Chance in the demonstration period, however, attendance and retention rates were high, due in large part to the staff's efforts to incorporate diverse learning strategies.

New Chance participants in Adams and Denver counties qualify for JOBS funding for child care and transportation support services, although the amount of child care funds continues to be inadequate and does restrict the number of teens who can be served. Also, JTPA funding has been used to pay for basic skills and occupational skills training and for tuition for the Living Competencies course.

In an effort to bring the New Chance services to more eligible teens who could not travel to TEC–North, the program was replicated in 1993 in another TEC campus. The Community College of Denver received a grant from JTPA to create a center in western Denver County (TEC–West) to serve 200 youth, including teen parents. The multiyear funding from JTPA, combined with grants from several other agencies and foundations, provides education and training services for the youth. An on-site child care center, similar to that at TEC–North, allows teen parents to participate in New Chance services, including the Living Competencies course.

As noted above, funding for child care remains a critical obstacle for serving more teen parents in both locations. While the community college has been able to secure education, training, and transportation services for teens, inadequate child care funding prevents either location from reaching maximum enrollment.

Adams and Denver counties and the community college remain committed to the New Chance model and hope to expand the reach of services to other parts of the counties as well. The development of proposals to foundations, state and federal agencies, and local organizations is an ongoing effort both to fill program funding gaps and to expand the number of teen parents served.

Since the conclusion of the demonstration period, the New Chance program has continued to educate and support young mothers aged 16 to 21 through three collegiate classes, worth a total of nine credits, in parenting, child development, and family health. An additional benefit provided for each New Chance student is the assignment of a case manager, who acts as a liaison with supporting agencies and provides education and career counseling.

The goal of New Chance remains to help young parents develop skills to become educated parents and learn self-development in such areas as self-esteem, decision making, and communication. These young parents also learn about domestic violence, relationships, child abuse, addictions, health issues, and furthering their education and culture awareness. As during the demonstration period, the learning takes place in small groups and classes that provide openness, confidentiality, and camaraderie. Field trips are scheduled to such places as museums, parks, and the zoo, where young parents learn to incorporate leisure activities into their and their children's lives. Additionally, a required parenting lab activity is held once a week, during which the young parents bring their children and engage in play activities as an interactive process. The parents are also required to do two hours of volunteer work per week in the child care center, to gain further experience in understanding children and their developmental levels. The New Chance classes are open entry, and tuition is provided through a federal grant.

Upon completion of the program, the New Chance students are presented with a certificate of completion and a personal letter of encouragement from the instructor.

Development Centers, Inc.
Community Mental Health Center
Detroit, Michigan

Development Centers, Inc. (DCI) was formed in 1983 from the dissolution and reorganization of two highly respected and long-standing community mental health centers in Wayne County. DCI provides mental health clinical and education services to children, adolescents, and adults residing in northwest Detroit and Redford Township.

DCI mounted a comprehensive support services program for high school–age parents in 1984. The program's on-site GED instruction, parenting education, developmental child care, individual and group counseling, and mental health services were important when DCI was assessed as a potential New Chance site. Although health education and services existed, they needed to be expanded to conform to the New Chance model. In addition, case management and employability development had to be added. The hiring of two case managers, a job developer, and a health educator, as well as linkages with local health care agencies, helped to address these issues.

Job-readiness training, referral to occupational skills training, and job placement assistance were available at DCI before the implementation of New Chance, but they were not core components of every participant's program experience. New Chance required DCI to move beyond its original concentration on education, parenting, and personal development to embrace economic self-sufficiency as a central objective for each participant. This shift in goals was accomplished through the efforts of DCI's executive staff, the New Chance project director and staff, and a local New Chance advisory group.

During the period under study, the project director and two case managers (all full-time staff of DCI) made up the core New Chance team. Their efforts were supported and complemented by staff from other DCI programs and outside agencies. DCI's parenting instructor and health educator worked part-time for New Chance. The Detroit public schools co-located two instructors at DCI to deliver individualized GED preparation and adult basic education. Classroom instruction was supplemented with practice activities in the Apple computer lab. Health education and services, work internships, vocational training, guest speakers, and field trips were available as a result of extensive outreach to community-based and public agencies.

A local advisory group guided and supported New Chance. The group helped persuade public and community-based agencies to provide services and other resources to New Chance, and funding development was an explicit part of its charge. The addition to the advisory group of the director of a Wayne County welfare office paved the way for referrals and other assistance from two additional county welfare offices. These referrals helped DCI to enroll 175 women. The assistance of this official has also enabled DCI to secure funds from Michigan's JOBS program.

Another important feature of DCI is its child development program. The child care center is licensed and has a full-time director. The child development program is a joint venture involving the child care, infant mental health, and parenting instruction staff, who carefully monitor parent/child interactions, intervene when necessary, and deliver consistent messages about appropriate parenting practices.

Poor attendance and attrition were the major operational challenges confronted by DCI during the demonstration period. Staff believed that many of those with poor attendance applied to the program only because they feared that welfare sanctions would be imposed if they did not enroll in an education or training program, as required by law. Yet those young women who were more positively motivated to enroll also faced obstacles to regular participation that included illness, housing problems, personal and family crises, and repeat pregnancies. To help participants overcome these obstacles, staff increased counseling, classroom instruction, and referrals to outside agencies.

In 1992, the prospects for institutionalizing New Chance at DCI were uncertain. There were a number of factors working in DCI's favor, however. DCI's executive leadership is solidly behind New Chance. The program received support from its congressional representative, staff from the state's human services agencies, and local political and community leaders. A variety of public and private funding sources were approached for support to continue New Chance operations, and DCI was awarded one of four county contracts to provide services to teen parents.

Since the end of the demonstration period, the site has continued to refine its services. It is now possible for enrollees to remain in Phase 1 for up to 12 months to give those who need it more time to develop their skills. DCI has also strengthened its support system for participants after they leave the program by providing a case manager to do follow-up. The parenting component has also been enhanced, with daily parent/child sessions at lunchtime.

Mid-Manhattan Adult Learning Center
Office of Adult and Continuing Education
New York City Board of Education
New York, New York

Located in the Harlem area of New York City, the Mid-Manhattan Adult Learning Center (MMALC) is one of several comprehensive adult learning centers operated by the New York City Board of Education's Office of Adult and Continuing Education. MMALC's New Chance program built on and integrated a sequence of services available at the school: GED, child care, life management, and prevocational courses and a wide variety of vocational training offerings. MMALC's participation in the New Chance Demonstration was co-sponsored by the New York State Department of Social Services. New Chance participants were co-enrolled in BEGIN, New York City's AFDC employment program operated by the Human Resources Administration (HRA), which funded transportation expenses and, when needed, off-site child care provided through the Agency for Child Development.

MMALC is well known for its intensive educational and vocational preparation. The school's reputation in these areas was a key factor in its selection as a demonstration site. The New Chance education, occupational skills training, and adult survival skills activities drew most heavily on MMALC's areas of expertise and experience and were among the New Chance program's strongest elements. Most of the services required by New Chance were already available at MMALC, and during the period under study several MMALC staff were brought into the program on a full- or part-time basis. In some instances, New Chance participants were in classes specifically designed for them, but they also attended classes with other MMALC enrollees. For example, although New Chance participants selecting occupational skills training programs at MMALC were placed in regular classes with other MMALC training students during the period, MMALC developed a separate education class for the New Chance participants.

MMALC's adult basic education and GED instruction were individualized and computer-assisted, but group instruction was also provided as a means of motivating students and helping them to develop reasoning and communication skills. Life management classes have been a part of the core curriculum at MMALC for many years. The instructor for the life skills class for New Chance participants used class discussions, audiovisual materials, field trips, and guest speakers to address the topics required in the New Chance health and personal development components, including legal and consumer issues, personal and family health matters, and citizenship and civic responsibilities. The content of MMALC's life skills class complemented New Chance's class in decision-making skills, which was added when New Chance was implemented.

Some services did have to be added or expanded to conform to the New Chance model; family planning education and case management services were added, and existing parenting, child care, and health education services were expanded. The Board of Education's Learning through Young Family Education (LYFE) program renovated and equipped two rooms at MMALC for use as an infant and toddler day care center. LYFE also provided licensed, trained staff for the center. The effort to add or expand services in the health and personal development components also drew on other agencies in the community. Family Dynamics Inc. conducted parenting classes, Planned Parenthood provided family planning workshops, and Harlem Hospital, through its community outreach effort, supplied guest speakers and a leader for health education workshops.

Phase II of New Chance included unpaid internships at government and community agencies, as well as paid work experience obtained through JTPA Summer Youth Employment Program positions. Case managers were responsible for helping GED recipients make the transition into skills training, assisting them as they decided what type of training they wanted to pursue and where they wanted to enroll. Only rarely were participants encouraged to enroll in skills training courses prior to passing the GED exam. MMALC was the principal provider for skills training courses for New Chance participants, although some who attained their GED through the program opted to enroll at community colleges.

During the first year of program operations, developing long-term linkage arrangements with outside agencies to provide New Chance's family planning and parenting components was challenging for the program. Staff changes at linkage agencies resulted in interruptions in service delivery and, in some instances, necessitated changes in linkage arrangements. Through the persistent efforts of program management, new staff or linkage agencies were found and integrated into the program. Staff turnover among MMALC staff teaching the New Chance GED class was another management concern.

Recruitment was also a challenge during the early period of program implementation. MMALC's strong relationships with other agencies in the community and with HRA, however, enabled it to marshal support for New Chance recruitment efforts and to meet the enrollment target, despite the fact that recruitment got off to a slow start and began later than planned because completion of the on-site infant care facility was delayed.

After the close of the demonstration period in December 1992, MMALC's New Chance program continued operating with funding from the New York City Board of Education and the New York State Department of Social Services, but with somewhat reduced staffing and enrollment.

During the 1993–1994 school year, MMALC placed a greater emphasis on integrating New Chance participants into its regular classes; New Chance enrollees had fewer separate classes and more classes with other MMALC students. An additional change during this period was increased use of MMALC's clerical and health occupation pre-vocational classes, which combined education classes and introductory, hands-on vocational training activities related to the career focus. New Chance participants scoring below 9.0 on the Tests of Adult Basic Education (TABE) reading test were placed in one of MMALC's two pre-vocational courses; enrollees scoring 9.0 and above were placed in the school's GED preparation class. Both classes had access to MMALC's newly developed parenting center for weekly parent/child literacy training and other interactive activities.

MMALC continues to provide educational and supportive services to teen parents, but the New Chance program ended in June 1994. Former New Chance students do return to MMALC to enroll or re-enroll in occupational skills or GED classes, and many former New Chance students continue to visit MMALC or call former New Chance staff to discuss their success and their problems.

Southern California Youth and Family Center
Inglewood, California

The Southern California Youth and Family Center (YFC), a nonprofit, community-based social service agency established in 1979, is located in Inglewood in Los Angeles County. YFC provides services in other locations as well, but services for New Chance participants (with the exception of occupational skills training and child care) were brought together under one roof at YFC's main site. Before the start of New Chance, YFC provided case management services to include counseling, parent and health education, and infant care for pregnant and parenting teens 18 years old and younger. In addition, the agency operated pregnancy prevention education and AIDS education programs in school districts in the area. In 1990, its programs served more than 300 young women, 50 teen fathers, and their families. The education program reached 4,000 students.

Although several of YFC's services needed to be strengthened and expanded for New Chance, the organization brought to the demonstration a dedicated, high-quality staff; a history of operating highly regarded, comprehensive programs for pregnant and parenting teens; experience in working with male partners of teenage mothers; a systematic approach to linking clients to needed services; and strong family life education and AIDS prevention programs.

In implementing New Chance, YFC collaborated formally with five major public institutions: the Inglewood Unified School District, the Los Angeles County Department of Public Social Services, the local Regional Occupational Center, the California Community College System, and the Inglewood JTPA agency. The Inglewood Unified School District provided the teaching staff for GED instruction and life skills curriculum at the site. The Los Angeles County Department of Social Services allowed YFC to recruit participants from AFDC and facilitated their enrollment in the local GAIN Program. Co-enrollment of New Chance participants in GAIN ensured that the California Department of Social Services would provide funding for support services such as child care, transportation, and some education- and training-related costs while the young women were in New Chance.

SCROC (Southern California Regional Occupational Center), a state-funded provider of education and occupational skills training programs, provided on-site word processing training to New Chance participants. New Chance staff in collaboration with SCROC staff were able to facilitate participants' enrollment in other occupational programs offered by the Center. JTPA provided funding for the purchase of the Comprehensive Competencies Program (CCP), a widely used computerized literacy, math, and GED preparation training system. CCP was installed at the site in late February 1991, and all New Chance staff received training in its operations. The system became the core of GED preparation for New Chance participants. JTPA was also a referral source for skills training programs. The New Chance staff collaborated with local community college staff to make sure that participants were oriented to the programs offered by the colleges, were able to take advantage of financial aid opportunities, and followed through with enrollment procedures and class work.

Building an integrated New Chance program in the Inglewood area presented special challenges to YFC staff. Many things were not within YFC's control. YFC had to devise a structure that would comply with the rules and constraints of GAIN and the school district and still be a personalized New Chance program. For example, because the Los Angeles GAIN program did not target teenage mothers for service during the time the research study group was recruited, neither the welfare agency nor GAIN referred eligible young women to New Chance. YFC staff were, however, permitted to recruit in person at the county's Income Maintenance office. Recruitment therefore occupied a great deal of staff time and attention, particularly for the case managers. In addition, the need to modify the policies and procedures that were targeted to older GAIN participants required major problem solving at all levels of operation. Other challenges involved integrating services provided by the school district. The YFC/New Chance staff had to negotiate with the school district for instructional staff. Also, the GED testing took place within the school district and on its schedule, so participants ready to take the GED exam often had to wait several weeks before the test was offered. Finally, in 1994, when the school district decided to eliminate some of its teaching positions from the budget, the New Chance program was one of its casualities.

Although YFC operated a child care facility, participants couldn't take advantage of this center because of limited space and an ongoing waiting list. Ten family day care providers were therefore recruited to serve the New Chance participants' children. YFC staff met with these child care providers regularly, both offering them support and training and receiving feedback on the children. But since the providers were spread out over a wide geographic area, YFC staff found it difficult to address parent/child issues with the participants in the day care settings; nor could they provide participants with practical feedback and input about their child in the day care setting.

Lastly, with several training providers, each with its own geographical limitation or operational complexities, tailoring skills training to participants' needs was difficult. One strategy YFC implemented to fill gaps in training programs' enrollment schedules was to offer internships in the interim between completion of the GED and the beginning of training. YFC was successful in recruiting businesses who provided internships for the participants.

YFC met these challenges, however, and was successful in putting together a high-quality New Chance program. By 1994, though, continuing difficulties with these bureaucracies, coupled with declining funding, overwhelmed the program, and YFC ceased program operations in the first quarter of the year.

The Bridge of Northeast Florida, Inc.
Jacksonville, Florida

The Bridge of Northeast Florida, Inc., formerly Family Health Services, Inc., was incorporated in 1972 as a consortium of providers for the purpose of addressing the medical and health care needs of low-income families. The Bridge, a model program providing comprehensive educational and social services for children and youth, was initiated in 1983 as a result of a Jacksonville Community Council study of teen pregnancy. From its inception, The Bridge has targeted children and youth of poor families where the risk is the highest for negative outcomes. In 1994, the corporate name was changed to The Bridge of Northeast Florida, Inc. to reflect more accurately its image and comprehensive range of services. The mission of the agency is "to promote the development of healthy, productive, self-sufficient youth and families by providing comprehensive health, social and educational programs." The programs of the organization have been conscientiously developed to meet the most critical needs of at-risk children and youth in the community. At present, in addition to New Chance, five distinct programs address these needs: The Bridge (education, recreation, and social and nutrition services for 950 children and youths aged 6 to 18, including a truancy program and a diversionary program for youth offenders with the State Attorney's Office); Juvenile Justice (intensive case management for 220 adjudicated youth aged 9 to 18); Ribault Full Service School (case management and anger control classes for youth aged 9 to 19); The Bridge Adolescent Clinic (medical services, including primary care immunizations and family planning for 1,200 adolescents and children); and Parents' Fair Share (a partnership with the state departments of Labor and Health and Rehabilitative Services to serve unemployed noncustodial parents of children receiving public assistance).

During the demonstration period, the New Chance program at The Bridge was co-sponsored by The Ounce of Prevention Fund of Florida, the local JTPA agency, the Private Industry Council (PIC) of Jacksonville, and Florida's Department of Health and Rehabilitation Services (HRS). Program services were offered in conjunction with the Florida Community College at Jacksonville (FCCJ). These co-sponsoring organizations have a history of collaboration in delivering employment services to the city's disadvantaged populations. The program is located at The Bridge of Northeast Florida's offices in an inner-city, predominantly black, residential neighborhood.

Broad experience in providing health, family planning, and parenting services to disadvantaged young mothers was one of the main reasons for selecting The Bridge to be part of the New Chance Demonstration, as was the history of collaboration among the agencies expected to help deliver component activities in areas in which The Bridge had less experience. Before New Chance, several types of activities and services were available to young mothers by appointment or on a drop-in basis, but The Bridge had not offered a comprehensive, daily program for this population. Mounting New Chance at this site involved adding new services, expanding the scope of existing activities, and shifting the agency's orientation to operating a full-time, comprehensive program.

The New Chance program built on The Bridge's strong parenting, family planning, and health education services. Other facilities and Bridge staff also became part of New Chance. The Bridge's on-site clinic provided prenatal and well-baby care, family planning counseling, and treatment for sexually transmitted diseases; other health services were provided at a nearby hospital.

While new staff were hired to provide case management, a different strategy was used to mount most of the additional services called for in the New Chance model. The Bridge developed a linkage with FCCJ to provide instructors for on-site adult basic education and GED classes. New Chance's employability development activities, another key component of the first phase of the program, were offered on-site through an arrangement with the local JTPA PIC, which assigned a staff person to the New Chance program.

The program's Phase II activities were also implemented with significant help from the collaborating agencies. A PIC staff person was instrumental in placing New Chance participants in JTPA paid work experience positions developed by and funded through the PIC. The program also used jobs available through JTPA's Summer Youth Employment Program as a way of offering paid work internships to participants. (Unpaid work internships were not offered during the demonstration period.) FCCJ offers a wide range of vocational courses at its nearby downtown campus and was the primary occupational skills training provider for New Chance participants. Tuition was mostly funded by federally provided Pell Grants, but tuition waivers were available to some participants through HRS. Job placement was handled by the same PIC staff person who led the program's employability development classes.

Child care, funded by HRS and the PIC, was available at nearby centers; one of the centers was for a time able to give priority to New

Chance participants. To qualify for HRS-funded support services, including child care and transportation, New Chance participants had to be co-enrolled in Project Independence, Florida's JOBS program.

The Bridge's linkage arrangements and the services delivered through them were exceptionally consistent. The agency's greatest challenge in implementing New Chance was to develop the structure for a full-time, intensive program for a hard-to-serve population. New Chance brought with it a host of management challenges—including creating and implementing incentive structures, rules, and attendance standards and developing staff consensus on expectations for participants and appropriate responses to their behavior—that are not typically encountered when services are by appointment or of limited duration, as had been usual at The Bridge before New Chance. The effects of irregular attendance on program services and participants' progress were a problem with which the site had little experience. As one way to address this problem, staff implemented an incentive program in which participants who met attendance requirements could earn points exchangeable for household items that could not be purchased with food stamps.

Since the end of the demonstration period, the site has continued to refine and augment services and implementation strategies. For example, the site has instituted unpaid work internships through a component it calls the Real-World Internships Project. Corporations and United Way agencies have responded to the New Chance program's effort to expand the number of internships it can offer through provision of short-term internships that, while unpaid, have nonetheless provided opportunities for participants to explore jobs and careers and to practice and reinforce skills learned in employability development classes. Other changes include integrating family planning into all other facets of the program and developing a bimonthly class that allows for hands-on parenting instruction, despite the lack of an on-site child care center. Participants bring their children with them to the program on Fridays, which—with the exception of the parent interaction class—are reserved for staff planning activities and meetings. The parenting instructor develops activities for the parents and children to do together while she observes and facilitates their interactions. In addition to receiving feedback from the instructor, participants also have an opportunity to observe the instructor model ways of encouraging positive development in children. In the site's continuing efforts to improve participants' attendance, hourly attendance is translated into payment vouchers that participants can redeem for donated goods at the program's "pantry."

The prospects for ongoing New Chance operations at this site appear good. There are continuing linkages to FCCJ for in-kind support for education and employment activities and to the state welfare employment program's Teen Parent Program. In addition, an ongoing funding arrangement has been developed with the Ounce of Prevention Fund, and the program has developed a linkage with the Duval County Extension Service for in-kind instructional services in parenting, health, and nutrition. Program staff are also pursuing additional sources of ongoing funding.

The Family Care Center
Lexington, Kentucky

The Family Care Center (FCC), a program of the Lexington-Fayette Urban County Government's Department of Social Services, Division of Family Services, had been designing a program to help AFDC recipients achieve self-sufficiency just as the New Chance Demonstration was evolving. When FCC opened in 1989, it replaced the Early Child Care Center, which had provided pediatric health care and social services to at-risk children. Because the Early Child Care Center had never operated a program specifically for teen parents, a New Chance program at this site was not able to build on an existing infrastructure. FCC was a good candidate for the demonstration, however, because plans were already under way there to build a multiservice center and operate a comprehensive program for AFDC recipients. Also, the commitment of the director of FCC, the staff of collaborating agencies, and the Urban County Government, as well as the support shown by the Cabinet for Human Resources (CHR)—Kentucky's state welfare agency—provided a compelling rationale for including FCC in the New Chance Demonstration.

FCC provides the comprehensive, multigenerational services required by the New Chance model. Including New Chance participants, FCC serves approximately 200 children and more than 100 teenage parents and AFDC recipients annually. The facility includes child development classrooms, adult classrooms, and observation rooms, along with a cafeteria, playroom, parent resource center, vocational assessment laboratory, computer laboratory, study area, library, and exercise room. The University of Kentucky staffs the comprehensive dental, preventive health (pediatric and adolescent), and medical care facilities located at FCC. An adolescent clinic is staffed by a University of Kentucky medical team one day each week.

Once FCC was selected to be a New Chance site, four case managers were hired. Contracts with the Fayette County Public School System, the local JTPA program, and other public agencies have allowed staff to be co-located at FCC to deliver education and employment-related instruction. The teachers used the New Chance guidelines to design their own curricula and instructional strategies, and the GED and adult basic education instructors mixed group and individualized instruction with computer-assisted instruction to create an innovative learning environment. FCC's parenting education, health services, and child care directors assumed responsibility for those aspects of New Chance, and FCC's child psychologist also provided support. Planned Parenthood offered family planning education and services to New Chance participants. While vocational skills training was not available on-site, participants had access to education and training programs offered by Lexington Community College, JTPA-funded agencies, and other training providers. Many local employers agreed to provide job-shadowing opportunities and work internships to New Chance participants.

New Chance continues to receive encouragement and support from CHR, the Lexington-Fayette Urban County Government, local public and private agencies, and a volunteer board, which helped the program gain widespread community support and helped staff fulfill the implementation and enrollment objectives and to develop a strategy for rewarding attendance and achievement. Family Care Center staff have continued to pursue solutions to implementation problems at the site. According to staff, participants' feelings of powerlessness and their lack of self-esteem and basic skills account for the repeat pregnancies at the Family Care Center; physical abuse and homelessness have also plagued many of the participants. Methods to overcome these obstacles to program success have included increased personal counseling, support groups co-led by case managers and mental health providers, referral to outside services, a high school diploma program that includes special education classes, and consistent monthly incentives for all students who have an attendance rate of 75 percent. Since 1993, the access to family planning services on site has helped reduce the repeat pregnancy rate to below 10 percent.

In 1993, the Family Care Center was selected as one of the four New Chance sites for a 17-month federal demonstration grant to enhance services and encourage a stronger relationship with the JOBS program. As a result, all 18- and 19-year-old parents on AFDC in Fayette County were mandated to be in an educational program. A JOBS case worker was assigned to the Family Care Center full-time to expedite any changes in benefits and support services for JOBS-eligible teen mothers while in the program, and to continue eligibility determination if mothers attend post-secondary activities for up to six months after employment. Bridge the Gap child care payments, which were initiated in November 1995, have made it possible for participants to make the transition to employment with no interruption in child care. The lack of direct payment for child care for AFDC recipients has been the major barrier to employment.

The Family Care Center continues to be an active partner with the state in planning services for teen parents. The JOBS program continues to fund four case managers and a clerical position. In February 1996, the Family Care Center received a state Family Preservation Grant to fund the Family Network. Evening sessions that include dinner and child care are offered to fathers, mothers, and extended family. The Life Skills and Opportunities component of the New Chance model was utilized in planning these eight-week sessions, which will be offered at least through June 1997. These state-provided resources, along with the support FCC receives from the Urban County Government, continue to make the long-term outlook for this New Chance program promising.

RESOURCE, Inc.
Minneapolis, Minnesota

RESOURCE, Inc. is a not-for-profit organization offering vocational rehabilitation, employment, chemical dependency, and mental health services to low-income individuals in Hennepin County and the city of Minneapolis. The New Chance program, which opened in 1989 at the Sabathani Center, a multiservice community center in south-central Minneapolis, serves only women who reside in the city.

New Chance is part of RESOURCE, Inc.'s Employment Action Center (EAC). The strength of EAC is its employment and training programs. By hiring experienced staff it was able to implement the New Chance health and personal development components and adult basic education classes. The program also created linkages to local organizations such as the Minneapolis School District for GED preparation and MELD for parenting instruction and staff training.

Since its inception in 1989, the program has evolved to its current model, which emphasizes employment and family stability. In a continuing effort to increase its accessibility to young families in the community, New Chance opened a North Minneapolis site, with space donated by the Hennepin County Children and Family Services Social Services Office. With two fully operational sites, New Chance is now accessible to young mothers in both North and South Minneapolis. In addition, New Chance is now operating four parenting groups.

The site has a strong relationship with STRIDE (Success Through Reaching Individual Development and Employment), Minnesota's JOBS program. STRIDE mandates participation in education programs for young mothers on welfare without a high school diploma or GED, and enrolling in New Chance is an option for fulfilling this requirement. The local STRIDE office helped New Chance staff to recruit for the program by providing lists of potentially eligible welfare recipients. New Chance staff also present information on the program at STRIDE orientation meetings and follow up any women who express an interest. In addition, STRIDE pays the training and child care costs associated with participation in New Chance, as well as a portion of the case managers' salaries.

The Minnesota Department of Human Services provided the initial state grant to RESOURCE, Inc. for the demonstration. RESOURCE, Inc. has successfully negotiated with the department to provide the program with continued funding, which allows the site to continue enrolling young women in New Chance. In addition, RESOURCE, Inc. was one of only three New Chance sites selected by the Administration for Children and Families of the U.S. Department of Health and Human Services to participate in a national demonstration of the effects of offering enhanced case management and home visits in conjunction with New Chance services.

Since the demonstration period ended, RESOURCE, Inc. has been able to continue to strengthen the New Chance program. Staff have added peer parenting counselors, a mentoring program, a weekly job club, early childhood family education, and more home visits by case managers, and have developed strong relationships with social service providers. The most recent addition has been a young father's program.

Lutheran Social Mission Society/Lutheran Settlement House
Women's Program
Philadelphia, Pennsylvania

The Lutheran Social Mission Society/Lutheran Settlement House (LSMS/LSH) is a nonsectarian, nonprofit community-based organization that is devoted to meeting the needs of disadvantaged children, youth, and women, and also of the elderly. Since 1976, one of its divisions, the Lutheran Settlement House Women's Program, has provided adult education (ABE/GED), vocational training, and services for victims of domestic violence. Located in Fishtown, one of Philadelphia's inner-city neighborhoods, many services of the Women's Program are targeted to low-income women and their families.

The New Chance program built on and operated as part of the existing Teen Parent Education/Employment Program (TPEEP), which the Women's Program had been operating since 1987. TPEEP enabled LSH to begin New Chance with quality education, parenting, and job-readiness services for adolescent parents already in place. With the addition of New Chance, however, the program's scope, duration, and size all changed. The program's focus was broadened to include employability development, preparation for vocational training, health, family planning, and life skills; the duration of the program increased from 4 to 6 months to up to 18 months; and the program's capacity doubled.

During the demonstration period, services were provided by Women's Program staff who worked full-time with the New Chance program and through linkages with outside organizations that were enlisted to enhance curriculum and services in the areas of health, family planning, parenting, and life skills. These linkage organizations included (but were not limited to) Jewish Family Services, Planned Parenthood, the Penn State Cooperative Extension Service, and an AIDS education peer-counseling program, all of which conducted workshops for New Chance on an ongoing basis.

Throughout the demonstration, the TPEEP/New Chance program also benefited from other historical linkages with agencies in the community. The TPEEP/New Chance staff mounted a successful recruitment campaign that enabled the program to reach its enrollment goal in less than a year through aggressive outreach and strong partnerships with local county assistance offices and JTPA staff. On several occasions, the local welfare department sent out recruitment letters to teen parents on its rolls. The good working relationships with caseworkers in local county assistance offices both supported the active participation of the teens enrolled in New Chance and generated new referrals. The site's relationship with the local JTPA agency, the Philadelphia Private Industry Council (PIC), which was the TPEEP/New Chance program's largest funder, was vital. The PIC was instrumental in helping to remove barriers faced by New Chance participants in making the transition to vocational training programs and also generated referrals for the program.

During the first year of the demonstration period, the small team of core TPEEP/New Chance staff managed to implement and operate all the components of New Chance and to keep the participants in the program despite recurring fiscal constraints, problems with the physical plant, and management changes in the parent organization. The staff were also challenged by the multiple difficulties facing many of the New Chance women, including physical or emotional abuse by family members or partners, drug and alcohol abuse in the young women's families, and unstable living arrangements.

During the demonstration period, the program staff demonstrated facility in integrating Phase I lesson content across New Chance subject areas and in making the information taught relevant to the young women's lives. The constant support and counseling provided by the case managers were a hallmark of the program, helping participants apply the life skills learned in New Chance to their day-to-day lives.

For New Chance, the program hired an employment specialist who coordinated all aspects of employment preparation and job placement. The specialist taught the program's employability development classes, arranged for internships and enrollment in skills training, and helped participants get jobs. Philadelphia had an advantage in that skills training providers were more likely to accept enrollees without a GED than was typical of many other New Chance communities; the specialist's efforts to move participants into training were hindered, however, by reluctance among many JTPA providers to risk serving teens. Their reluctance mainly stemmed from their lack of understanding about the New Chance program's ability to help support and monitor New Chance enrollees while they attended training classes, and the program's persistent appeals to the PIC eventually improved the participants' access to courses and led to better communication between the New Chance program staff and the training providers.

Since the publication of the 1994 report on the implementation of New Chance, the site has incorporated a greater emphasis on helping participants make the transition into skills training and has significantly increased the proportion of enrollees who participate in vocational courses, exceeding in most years the goals set in the site's Single Point of Contact (SPOC) contract. These changes were initiated by the program's principal funding source, the SPOC program, which is part of Pennsylvania's JOBS program. In line with SPOC policy, there is less emphasis on GED attainment, and the goal of the program's education classes is seen more as the achievement of educational benchmarks and enhancements (including, where appropriate, the GED) needed to enter skills training. Another change in program services since the end of the demonstration is the inclusion of an ongoing group counseling workshop led by a psychologist. The weekly sessions focus on mental health issues of concern to participants. LSMS/LSH continues to maintain the integrity of the New Chance model. Any new enhancements are considered and developed on the basis of how they will fit with the model's core services.

During the demonstration, the TPEEP/New Chance program was supported by funds from the SPOC Program, supplemented by contributions from private and corporate foundations. All New Chance participants receiving AFDC were enrolled in SPOC. The program has continued to receive SPOC funding beyond the operational phase of the demonstration and has been able to maintain both the additional programmatic components and the enrollment levels achieved during the demonstration.

Young Mothers Program
The Hill House Association
Pittsburgh, Pennsylvania

The Hill House Association (HHA) is a multiservice, community-based agency that has served residents of Pittsburgh's Hill district since 1964. New Chance is a component of The Hill House Association's Young Mothers Program, one of several programs under The Hill House Association's Department of Education and Support Services (HHA/DESS), formerly known as Pittsburgh in Partnership with Parents. Started in 1985, the program offers educational and employment opportunities to young parents. The Young Mothers Program operates under the management of the HHA executive director and the HHA/DESS director and is located on the agency's premises. Direction is also provided by an advisory committee composed of representatives from the city's public, private, philanthropic, and not-for-profit sectors. The Hill House Association's location in the heart of the Hill district, one of the city's oldest historically black inner-city neighborhoods, accounts for the program's predominantly black enrollment.

As one of the six agencies to participate in the national pilot phase of the New Chance program, The Hill House Association entered the demonstration with substantial operating experience. All the model's components were in place at the beginning of the demonstration, and HHA's challenge was to refine implementation of activities and objectives. At the conclusion of the New Chance pilot, The Hill House Association revised its intensive, almost exclusive focus on education during the early months of an enrollee's participation in New Chance to permit a greater concurrent emphasis on employability and personal development activities. In addition, employability development was restructured to include an intensive career exploration phase following GED receipt. To support these changes, The Hill House Association developed its own curriculum guide for both education and employment-preparation activities.

During the demonstration period, the sequence of activities specifically aimed at preparing participants for employment was one of The Hill House Association's strongest program elements. While HHA's approach shared some of the characteristics of strategies used at other New Chance sites, there were important differences. As at other sites, participation in employment-related activities began at program entry, with participation in introductory career-exploration classes led by the training specialist, and job-readiness classes led by the program's job developer. Participation in these activities continued as students progressed toward taking the GED test. In contrast to other New Chance programs, however, the HHA program scheduled intensive examination of career possibilities, including work internship and job-shadowing opportunities, during a multiweek career-exploration program that followed GED receipt. Courses in this phase were again largely the responsibility of the training specialist. At the completion of this phase, the specialist helped each participant select a career area and a vocational training course and was responsible for monitoring her progress. Following the completion of training, the job developer assisted participants in obtaining employment.

While Pittsburgh enrollees faced many of the same problems as those at other sites, perhaps the greatest ongoing challenge to the site was managerial: that of integrating staff from a variety of agencies. The Hill House Association may well represent the demonstration's strongest example of a program in which almost all New Chance services were delivered on-site but were also brokered. Only administrative, case management, and employability development services were provided by staff entirely on The Hill House Association payroll; all other activities were conducted by full- or part-time staff from collaborating agencies under in-kind or contractual arrangements. To foster program cohesiveness, staff participated in an annual retreat held each summer to review the prior year's progress, successes, and disappointments. These retreats supplemented routine monthly staff meetings.

Since the end of the demonstration, the Pittsburgh program has concentrated on enhancing the education and parenting services with services more specifically focused on direct employment placement. The Young Mothers Program recently added a state-of-the-art Windows-based computer lab for GED preparation and employment skill enhancement. Also, participants have biweekly parent/child educational field trips and activities to promote the concepts discussed in the parenting classes. Another change has been a greater emphasis on the JTPA youth competencies as an outcome of the Young Mothers Program's employability development classes. At the end of each cycle, JTPA staff have been asked to help evaluate participants' attainment of the competencies through assessment of their class portfolios and demonstrated skills. Finally, to comply with state funding requirements, the Young Mothers Program currently moves participants to skills training courses within six months of program entry.

Many significant changes also occurred in HHA's Young Fathers Program. The Hill House Association has strengthened its program for young fathers— which, although integrated with the Young Mothers Program during the demonstration, now has a separate staff and facility—by creating a distinct education, employment preparation, and parenting curriculum designed specifically to address the issues of the young men.

The Hill House Association's young parents programs are well institutionalized and enjoy wide support at the local, state, and national levels. The Single Point of Contact (SPOC) Program—part of Pennsylvania's JOBS initiative—is the Young Mothers Program's principal source of funding, and all New Chance participants who receive AFDC are enrolled in it. The continuation of the program as implemented during the New Chance demonstration seems secure, as most of the program's demonstration-related SPOC funding remains in place.

PIVOT–New Chance Program
Portland Public Schools
Portland, Oregon

The New Chance program in Portland is a joint effort of the Portland Public Schools and the Portland Job Corps. New Chance, which is known as PIVOT (Partners in Vocational Opportunities Training), is an outgrowth of the school district's Continuing Education for Girls (CEG) program. CEG, now known as Monroe, has been operating for 19 years as an alternative educational setting serving pregnant students, who usually return to their home schools at some point after the birth of their children. Monroe students tend to be younger than those enrolled in New Chance and usually are not high school dropouts. Monroe offers an accelerated high school curriculum and a GED-preparation curriculum, as well as parenting, health, and counseling services.

CEG formed a partnership with the Portland Job Corps to implement the full New Chance model. CEG provided education, health, and personal development services, and the Job Corps—with special federal funds—provided the employability development and occupational skills training services, stipends, and support services. Drawing on CEG and Job Corps personnel, PIVOT–New Chance developed one of the largest staffs in the demonstration. The creation of a special staff position to recruit and enroll New Chance participants allowed the site to increase its enrollment goal. In addition, the program was one of the first to recognize the range of participants' problems calling for outside intervention; accordingly, it arranged for the Oregon Health Services University to provide services, including mental health counseling, at the on-site health clinic and for treatment for substance abuse to be provided through a community drug and alcohol program.

There are several notable features of the PIVOT–New Chance services. The GED classes have been strengthened by the use of computer-assisted instruction, which gives the teacher more time to work individually with students, and pre-GED classes are provided through literacy labs. The program also offers an on-site, two-semester business skills training course that includes word processing, typing, "10-key" (use of an adding machine) and record keeping, business English, and telephone skills training. Advanced career training is offered through the Mount Hood and Portland community colleges. In addition, on-site child care is available at the Head Start Infant and Toddler Center. Van transportation is provided by the Job Corps, and monthly bus passes are provided by Adult and Family Services (AFS) for participants receiving cash assistance. Working closely with AFS, the site now has an AFS teen parent case worker on-site to "enhance service delivery." Enrichment courses in life skills are also provided through the Job Corps.

The greatest challenge facing the collaboration in Portland has been fulfilling the different program requirements, streamlining them for administrative effectiveness, and recognizing the varied organizational philosophies of the agencies involved in implementing and operating New Chance. Development of a joint management structure has allowed input from the primary agencies involved, including MDRC, to ensure that each agency's goals and requirements are met. The program is a model collaboration program that utilizes curricula provided through MDRC, the Job Corps, and the Portland Public Schools. Its structure and community linkages have been strategically designed to enhance service delivery and funding opportunities. In working with its collaborative partners, it continues to strive for (1) shared vision, (2) shared mission, and (3) shared resources, which its director defines as the keys to its success.

Through the efforts of the Job Corps and Monroe staffs, community awareness of the program has grown. For example, PIVOT Pals, a network of businesses, ensures regular donations of money, goods, and services to the program through such activities as sponsoring monthly awards luncheons for participants at local restaurants; providing work experience opportunities, telephone skills training, and job placements; and collecting gift certificates for use as program incentives. Students have also been featured in television news stories and newspaper articles.

The Portland New Chance program was also one of ten national programs to receive a 17-month grant through the U.S. Department of Health and Human Services.

Oregon's JOBS program, which began in October 1990, emphasizes enrolling young mothers on welfare in high school or GED programs. Consequently, Portland Public Schools has a contract with the local JOBS program to identify young mothers who have not finished high school, assess their need for services, and facilitate enrollment in one of several district program options, including PIVOT–New Chance. The JOBS program also provides child care funding for PIVOT–New Chance enrollees. In addition, PIVOT–New Chance staff are involved in the local welfare office's planning committee for JOBS services to teens.

Teen Parent Program
The YWCA of Salem
Salem, Oregon

The YWCA of Salem is an affiliate of the national service organization and serves residents of Salem and the neighboring counties. The YWCA has operated the Teen Parent Program for 27 years, providing such services as education, employment skills training, and child care. The program, which included New Chance during the research project, moved to new facilities at the Oregon School for the Deaf in 1990. The building it occupies, a former dormitory, has classrooms, meeting rooms, offices, and a child care center.

When the YWCA was chosen for the demonstration, the education, parenting, health education, and counseling services were the backbone of its Teen Parent Program. For New Chance, the site increased its emphasis on employability development and attainment of a GED. The YWCA already had linkages with the Salem/Keizer (24J) School District, Chemeketa Community College (CCC), and the Marion County Public Health and Mental Health departments, which all became service providers for New Chance participants as well. Because the YWCA also worked with the state's Executive Department as part of a business/school partnership program, Executive Department staff volunteered to become mentors for New Chance participants. The department also accepted New Chance students into its clerical training courses on a space-available basis, and members hosted holiday dinners and donated clothing and toys to the participants.

Most New Chance staff were employed by the YWCA. The GED instructor was provided by CCC. The YWCA was one of five New Chance sites to receive a donation of Apple computers to develop a computer learning center, which was used for GED instruction and employability development activities. After participants completed the GED course, they could enroll at CCC for occupational skills training courses. Job placement assistance was provided at the Teen Parent Program by an employment teacher and through the Mid-Willamette Valley Jobs Council (the local JTPA agency).

The YWCA operates a child care center for infants and toddlers at the site, and New Chance participants received priority for its full-time services. Participants were also co-enrolled in the public school district, allowing them to receive free lunch and transportation services.

Relative to other New Chance programs, the Salem site had a small number of potentially eligible young women in the area, but the staff's persistence in recruiting enabled the site to meet its enrollment goal by June 1991. Securing steady attendance and retaining enrollees were major challenges, however. Efforts were made to re-enroll participants who dropped out of New Chance because of health and family problems. Another issue was turnover among program personnel at both the managerial and instructional levels.

JOBS was implemented in the Salem area in October 1991. When the local welfare office contracted with Chemeketa Community College to provide services to welfare clients, the YWCA's linkage with CCC became even more important. Site staff's meetings with CCC and welfare office staff facilitated New Chance access to skills training for participants. In addition, the YWCA worked with CCC to help New Chance participants gain greater access to the occupational skills training and job search programs at the college.

The Teen Parent/New Chance program has now been institutionalized within the local School District 14J as Barbara Roberts High School (named after a former governor). All facets of the New Chance program continue, and the JOBS program is working well in partnership with Chemeketa Community College. The school district is now the lead agency in the partnership. Chemeketa continues to provide GED and counseling, Marion County continues health services, the State of Oregon Adult and Family Services agency provides case management and access to JOBS-funded training classes, and the YWCA operates the on-site child care center for infants and toddlers. The state provides double school support for each teen parent, and there is a great deal of support for the program within the partner agencies.

Independence Adult Center
East Side Union High School District
San Jose, California

In 1988, the Independence Adult Center merged with the East Side Union High School District. With state funds, the district provides subsidized child development and child care services to low-income families and children at risk of neglect and abuse. It also operates preschool programs on seven high school campuses and—at a separate facility—the Family Learning Center, which provides child care and support services to in-school pregnant and parenting teens through age 17. The Family Learning Center enjoys a statewide reputation for excellence. Although implementing New Chance required the Independence Adult Center to start a new program, it had many services already in place on which to build, and staff members were excited by the opportunity to expand existing services to serve an older population. The Independence Adult Center serves all adults who apply, many of whom meet New Chance eligibility criteria.

Approximately 6,000 youths drop out of school annually on San Jose's East Side. In addition, a large proportion of the teen births in Santa Clara County are to residents of this area. In 1988, for example, there were 2,170 births to females between the ages of 11 and 19, accounting for 54 percent of all teen births in the county. The East Side is also home to 80 percent of the county population eligible for Greater Avenues for Independence (GAIN), the state JOBS program.

Staffing New Chance was a major challenge during the demonstration period, given the strict hiring requirements imposed by the school district, but the program assembled a strong and cohesive staff. To provide all the services required by the New Chance model, the East Side site also negotiated linkage agreements for occupational skills training with the San Jose Job Corps, the Central County Occupational Program, and the Center for Employment Training. Many participants attended local community colleges for training as well.

The most significant linkage agreement for East Side is its arrangement with the Santa Clara County GAIN program, which worked extensively with the site to change the local GAIN contracting procedures and program flow to facilitate enrollment of New Chance participants. During the demonstration period, GAIN held special orientation sessions for potential New Chance applicants and referred new GAIN registrants to the program. New Chance participants who are co-enrolled in GAIN have child care, transportation, and GED books and tests paid for by GAIN, which also provides additional money for training materials or for tools and uniforms required for a job.

East Side faced the same challenges as most programs for hard-to-serve populations during the study period, especially in regard to participant punctuality, attendance, and retention. The site addressed these issues by using various "carrots" and "sticks." Participants received breakfast and lunch every day. A peer counseling program for which the participants elected the counselors was started. There were many field trips and regular awards luncheons honoring those participants who, for example, had received or made progress toward receiving a

GED, had shown a significant change in attitude, had near-perfect attendance, or were the best students "all around." The program coordinators also instituted a "Lucky Bucks" incentive program, whereby participants earned credits for being punctual, demonstrating leadership, volunteering to help others, and recruiting new applicants. These credits could be used to buy baby products, cosmetics, and toiletries from East Side. The attendance policy (participants must attend 65 percent of all scheduled classes) was strictly enforced, and there was a one-month probation period for those who did not meet the requirement.

Since the end of the demonstration period, reduced funding has resulted in significant changes at this site. First, beginning in 1993, East Side reduced the number of participants served each year by about 25 percent. Second, beginning also in that year, in accordance with an agreement with the Santa Clara County GAIN program, participants returned to their GAIN case managers after they completed the first phase of the program, which includes the education, life skills, parenting, family planning, and employability services. The GAIN case managers referred them to skills training centers or community colleges.

Since September 1996, the site has been serving only young mothers in the state's new CAL-LEARN program, which provides case management and education services to young mothers 17–19 years old who receive cash assistance and who lack high school diplomas. Significant funding reductions have reduced other New Chance Phase I services to a minimum, but New Chance case management services provide continuous guidance for participants as they complete the CAL-LEARN education requirements and move into GAIN's skills training classes or into employment.

Appendix G

Table G.1

Cumulative Rates of Participation and Number of Weeks of Participation in Adult Education, College, and Skills Training by New Chance Control Group Members Within 42 Months After Random Assignment

	Ever Participated in (%) (Figure 3.1)			Weeks Participated in (Figure 3.2)			
Follow-Up Period	Adult Education (ABE/GED)	College	Skills Training	Adult Education (ABE/GED)	College	Skills Training	Adult Education, College, or Skills Training
Month 1	9.0	0.6	1.8	0.3	0.0	0.1	0.4
Month 2	14.7	1.0	2.9	0.8	0.1	0.2	1.0
Month 3	20.2	1.2	4.6	1.4	0.1	0.3	1.9
Month 4	24.5	2.2	6.0	2.2	0.2	0.5	2.9
Month 5	28.2	2.8	8.0	3.0	0.3	0.8	4.1
Month 6	31.4	3.1	9.4	3.8	0.4	1.1	5.3
Month 7	34.4	3.7	11.1	4.6	0.6	1.4	6.5
Month 8	36.6	4.7	12.1	5.3	0.7	1.7	7.8
Month 9	39.7	5.3	12.8	6.1	0.9	2.0	9.1
Month 10	41.9	5.8	13.7	6.9	1.1	2.4	10.4
Month 11	43.1	6.2	14.3	7.7	1.3	2.7	11.6
Month 12	44.1	6.5	14.7	8.3	1.4	3.0	12.8
Month 13	45.9	7.1	16.4	8.9	1.6	3.4	13.8
Month 14	47.5	7.2	17.1	9.5	1.7	3.7	14.9
Month 15	49.4	7.5	18.1	10.0	1.9	4.0	15.9
Month 16	51.2	8.0	19.5	10.5	2.1	4.4	17.0
Month 17	53.1	8.3	20.5	11.0	2.2	4.7	18.0
Month 18	54.9	8.7	21.5	11.5	2.4	5.0	18.9
Month 19	57.1	9.7	22.7	12.0	2.6	5.2	19.8
Month 20	57.2	10.2	23.3	12.4	2.8	5.4	20.6
Month 21	58.1	10.8	24.0	12.7	3.0	5.6	21.3
Month 22	58.4	11.4	24.3	13.0	3.2	5.8	22.0
Month 23	59.3	11.8	25.1	13.3	3.3	6.0	22.7
Month 24	59.6	12.5	26.4	13.7	3.5	6.2	23.4
Month 25	59.7	13.3	26.8	14.0	3.8	6.4	24.1
Month 26	60.3	13.6	28.2	14.2	4.0	6.6	24.8
Month 27	60.8	14.2	28.6	14.5	4.2	6.8	25.5
Month 28	60.9	14.5	29.1	14.7	4.5	7.0	26.2
Month 29	61.2	14.9	29.8	15.0	4.7	7.2	26.9
Month 30	61.7	15.3	30.2	15.2	5.0	7.4	27.6
Month 31	61.9	15.3	31.1	15.5	5.2	7.6	28.3
Month 32	62.2	15.6	31.7	15.7	5.4	7.8	29.0
Month 33	63.0	16.1	32.4	16.0	5.6	8.1	29.7
Month 34	63.6	16.2	33.0	16.2	5.9	8.3	30.4
Month 35	64.6	16.8	33.5	16.5	6.1	8.5	31.1
Month 36	65.2	16.8	34.2	16.8	6.3	8.7	31.8
Month 37	65.8	17.3	34.7	17.1	6.5	8.9	32.5
Month 38	66.1	17.4	35.1	17.3	6.8	9.1	33.2
Month 39	67.3	18.0	35.8	17.6	7.0	9.3	33.9
Month 40	68.3	18.6	36.3	18.0	7.2	9.5	34.7
Month 41	69.0	19.3	37.5	18.3	7.4	9.7	35.5
Month 42	69.5	19.9	38.1	18.6	7.7	9.9	36.2
Sample size 678							

(continued)

Table G.1 (continued)

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for all 678 control members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes.

The numbers in this table are not regression-adjusted. Therefore, the numbers in this table may not match exactly with the numbers in previous tables.

For controls, services were obtained at programs or agencies other than New Chance.

Rounding may cause slight discrepancies in sums and differences.

Table G.2

Receipt of Education Credentials by New Chance Control Group Members Within 42 Months After Random Assignment (Figure 3.3)

Follow-Up Period	High School Diploma (%)	GED Certificate (%)	Training Certificate (%)	No Education Credential (%)
Month 1	7.5	2.1	6.2	85.1
Month 2	7.5	2.8	6.2	84.4
Month 3	7.5	3.7	6.2	83.5
Month 4	7.7	4.9	6.3	82.0
Month 5	7.7	6.1	6.8	80.7
Month 6	7.8	8.2	7.1	78.5
Month 7	7.8	9.1	7.7	77.0
Month 8	8.0	9.8	7.8	76.0
Month 9	8.0	10.5	8.4	74.8
Month 10	8.0	12.3	8.8	72.7
Month 11	8.1	12.8	9.4	71.8
Month 12	8.6	14.7	9.7	69.9
Month 13	8.7	14.7	10.5	68.7
Month 14	9.0	15.8	11.1	67.4
Month 15	9.1	16.9	11.5	66.4
Month 16	9.1	17.5	12.1	65.8
Month 17	9.1	18.7	13.3	64.3
Month 18	9.6	21.1	14.0	61.9
Month 19	9.6	21.3	14.7	61.1
Month 20	9.6	21.8	5.6	60.6
Month 21	9.6	22.1	5.9	60.3
Month 22	10.0	22.5	15.2	59.1
Month 23	10.0	22.8	15.8	58.7
Month 24	10.0	23.5	15.8	58.1
Month 25	10.0	24.0	16.1	57.7
Month 26	10.2	25.0	17.0	55.8
Month 27	10.2	25.6	17.8	54.9
Month 28	10.3	25.6	18.0	54.9
Month 29	10.6	25.9	18.1	54.4
Month 30	10.8	26.7	19.0	53.7
Month 31	10.8	27.4	19.6	52.8
Month 32	10.8	28.2	20.1	52.1
Month 33	10.8	28.3	20.5	51.9
Month 34	10.9	28.6	20.8	51.5
Month 35	10.9	29.1	21.2	50.9
Month 36	10.9	29.5	21.8	50.3
Month 37	11.1	30.3	22.6	49.3
Month 38	11.1	30.4	23.0	49.1
Month 39	11.1	30.8	23.2	49.0
Month 40	11.1	31.3	23.6	48.5
Month 41	11.1	31.9	23.9	47.9
Month 42	11.1	32.5	24.6	47.1
Sample size 678				

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.1.

Table G.3

Cumulative Rates of Pregnancy and Birth for New Chance Control Group Members Within 42 Months After Random Assignment (Figure 3.5)

Follow-Up Period	Percentage with a Pregnancy	Percentage with a Birth
Month 1	8.7	0.1
Month 2	11.5	0.3
Month 3	15.6	0.3
Month 4	18.6	0.6
Month 5	23.2	0.9
Month 6	26.3	1.5
Month 7	28.3	1.9
Month 8	31.9	3.2
Month 9	33.5	5.2
Month 10	35.8	6.9
Month 11	38.6	9.3
Month 12	40.9	10.9
Month 13	43.5	13.9
Month 14	45.1	16.2
Month 15	47.6	18.4
Month 16	49.7	20.2
Month 17	51.2	22.1
Month 18	52.7	23.6
Month 19	53.8	25.2
Month 20	54.9	27.0
Month 21	55.8	28.8
Month 22	56.5	30.1
Month 23	58.1	31.9
Month 24	59.4	34.1
Month 25	60.6	35.7
Month 26	61.8	37.6
Month 27	62.7	39.2
Month 28	63.4	40.9
Month 29	64.3	41.7
Month 30	64.7	42.8
Month 31	66.4	44.2
Month 32	67.7	45.3
Month 33	68.4	47.2
Month 34	69.0	48.4
Month 35	69.3	49.3
Month 36	70.1	50.3
Month 37	70.8	51.2
Month 38	71.2	51.8
Month 39	71.5	52.8
Month 40	72.3	54.4
Month 41	72.7	54.9
Month 42	73.0	55.6
Sample size 678		

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.1

Table G.4

Monthly Full-Time and Part-Time Employment Rates for New Chance Control Group Members Within 42 Months After Random Assignment (Figure 3.6)

Follow-Up Period	Worked Full-Time (%)	Worked Part-Time (%)	Any Work (%)
Month 1	3.8	3.2	7.1
Month 2	4.7	4.0	8.7
Month 3	5.5	5.5	10.9
Month 4	6.3	5.8	12.1
Month 5	8.1	5.6	13.7
Month 6	9.4	6.6	16.1
Month 7	9.4	5.8	15.2
Month 8	9.9	5.6	15.5
Month 9	11.2	4.9	16.1
Month 10	9.3	6.0	15.3
Month 11	9.7	6.5	16.2
Month 12	10.0	6.3	16.4
Month 13	10.2	7.1	17.3
Month 14	10.3	8.7	19.0
Month 15	10.3	8.6	18.9
Month 16	11.4	8.4	19.8
Month 17	11.4	8.4	19.8
Month 18	11.2	8.0	19.2
Month 19	11.5	8.1	19.6
Month 20	12.4	5.6	18.0
Month 21	13.6	5.9	19.5
Month 22	12.5	5.3	17.8
Month 23	12.1	6.2	18.3
Month 24	13.1	5.9	19.0
Month 25	15.2	5.3	20.5
Month 26	15.6	5.9	21.5
Month 27	16.5	6.3	22.9
Month 28	18.1	7.5	25.7
Month 29	17.7	8.4	26.1
Month 30	18.4	8.1	26.5
Month 31	19.2	8.1	27.3
Month 32	20.4	8.6	28.9
Month 33	22.0	8.1	30.1
Month 34	21.7	8.8	30.5
Month 35	22.0	8.6	30.5
Month 36	22.6	8.1	30.7
Month 37	23.0	8.1	31.1
Month 38	23.6	8.7	32.3
Month 39	23.9	9.4	33.3
Month 40	24.0	9.7	33.8
Month 41	23.0	11.2	34.2
Month 42	23.6	9.6	33.2

Sample size 678

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.1. Part-time employment is defined as working fewer than 30 hours per week. Full-time employment is defined as working 30 hours per week or more.

Table G.5

AFDC Receipt by New Chance Control Group Members Within 42 Months After Random Assignment (Figure 3.7)

Follow-Up Period	Monthly Percentage Receiving AFDC	Cumulative Percentage Ever Left AFDC
Month 1	92.9	7.1
Month 2	91.9	8.8
Month 3	91.0	10.0
Month 4	89.4	12.2
Month 5	88.3	13.9
Month 6	88.3	14.6
Month 7	87.0	16.2
Month 8	87.0	17.3
Month 9	86.7	18.3
Month 10	87.0	18.7
Month 11	87.0	19.3
Month 12	86.7	20.4
Month 13	86.6	21.1
Month 14	86.1	22.1
Month 15	86.3	23.3
Month 16	86.1	24.3
Month 17	86.3	24.9
Month 18	86.0	26.0
Month 19	87.0	26.8
Month 20	85.3	28.5
Month 21	84.4	29.9
Month 22	83.5	31.1
Month 23	83.3	31.9
Month 24	82.3	32.7
Month 25	81.9	33.6
Month 26	81.4	34.1
Month 27	81.7	34.4
Month 28	81.1	35.1
Month 29	80.8	35.7
Month 30	80.4	36.6
Month 31	79.9	37.0
Month 32	78.3	38.1
Month 33	77.6	38.8
Month 34	77.3	39.8
Month 35	77.1	40.6
Month 36	76.5	41.4
Month 37	75.1	42.5
Month 38	74.0	43.7
Month 39	74.6	44.2
Month 40	75.1	45.0
Month 41	74.8	45.6
Month 42	73.9	47.1
Sample size 678		

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.1.

Table G.6

Cumulative Rates of Pregnancy and Birth for New Chance Sample Members Within 42 Months After Random Assignment (Figure 6.1)

Follow-Up	Pregnancies (%)				Births (%)			
Period	Experimentals	Controls	Difference	p^a	Experimentals	Controls	Difference	p^a
Month 1	11.0	8.5	2.6 *	0.070	0.1	0.2	0.0	0.862
Month 2	14.5	11.2	3.3 **	0.039	0.1	0.3	-0.2	0.380
Month 3	17.1	15.3	1.8	0.292	0.2	0.3	-0.1	0.617
Month 4	19.9	18.2	1.7	0.350	0.4	0.6	-0.2	0.428
Month 5	22.5	22.8	-0.2	0.901	0.9	0.9	0.0	0.964
Month 6	26.6	25.9	0.7	0.720	1.3	1.4	-0.1	0.779
Month 7	29.8	28.0	1.8	0.398	2.5	1.8	0.7	0.314
Month 8	32.9	31.6	1.3	0.563	3.8	3.1	0.7	0.398
Month 9	36.0	33.2	2.8	0.204	5.7	5.0	0.7	0.479
Month 10	38.9	35.6	3.3	0.142	7.5	6.8	0.7	0.549
Month 11	41.4	38.5	2.9	0.203	9.9	9.1	0.8	0.562
Month 12	43.5	40.7	2.9	0.211	11.4	10.7	0.7	0.619
Month 13	46.6	43.3	3.3	0.151	14.2	13.6	0.6	0.705
Month 14	49.1	44.9	4.2 *	0.069	16.2	16.0	0.2	0.891
Month 15	51.3	47.3	4.0 *	0.085	18.6	18.2	0.4	0.822
Month 16	53.4	49.4	3.9 *	0.087	20.6	20.0	0.6	0.743
Month 17	54.7	50.9	3.7	0.105	22.6	21.8	0.7	0.700
Month 18	55.9	52.3	3.6	0.119	24.4	23.3	1.1	0.569
Month 19	57.5	53.5	4.0 *	5.600	25.8	24.9	0.9	0.648
Month 20	58.5	54.6	4.0 *	5.900	27.4	26.6	0.8	0.684
Month 21	59.7	55.5	4.2 *	0.066	29.1	28.4	0.7	0.732
Month 22	60.3	56.2	4.1 *	0.069	31.2	29.7	1.6	0.466
Month 23	61.1	57.9	3.2	0.155	33.6	31.5	2.1	0.343
Month 24	62.4	59.2	3.2	0.152	35.0	33.6	1.4	0.517
Month 25	63.7	60.4	3.2	0.145	36.9	35.3	1.6	0.470
Month 26	64.7	61.7	3.0	0.178	37.9	37.2	0.7	0.740
Month 27	65.9	62.6	3.4	0.127	38.8	38.8	0.1	0.967
Month 28	66.5	63.3	3.2	0.142	39.6	40.3	-0.8	0.733
Month 29	67.4	64.2	3.3	0.135	41.0	41.3	-0.3	0.911
Month 30	68.2	64.6	3.5	0.105	42.2	42.4	-0.2	0.923
Month 31	68.7	66.3	2.4	0.264	43.0	43.9	-0.9	0.688
Month 32	69.4	67.6	1.8	0.390	44.2	45.0	-0.8	0.731
Month 33	70.1	68.4	1.7	0.421	45.9	46.8	-0.9	0.687
Month 34	70.6	68.9	1.7	0.423	47.3	48.0	-0.8	0.735
Month 35	71.2	69.3	1.9	0.360	48.4	49.0	-0.6	0.781
Month 36	72.0	70.0	2.0	0.337	49.1	50.1	-1.0	0.663
Month 37	72.4	70.7	1.7	0.409	50.2	50.9	-0.7	0.755
Month 38	72.9	71.1	1.8	0.386	51.2	51.5	-0.3	0.888
Month 39	73.7	71.4	2.3	0.269	51.8	52.6	-0.8	0.740
Month 40	74.4	72.1	2.2	0.269	52.6	54.2	-1.6	0.480
Month 41	74.8	72.6	2.2	0.267	53.9	54.6	-0.7	0.744
Month 42	75.2	72.8	2.3	0.246	54.7	55.3	-0.7	0.767
Sample size	1,401	678			1,401	678		

(continued)

Table G.6 (continued)

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

The averages or percentages are adjusted using linear analysis of covariance procedures controlling for up to 51 kinds of differences in characteristics before random assignment. Rounding may cause slight discrepancies in sums and differences.

[a]A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table G.7

Monthly Employment Rates and Average Monthly Earnings for New Chance Sample Members Within 42 Months After Random Assignment (Figures 7.1 and 7.5)

Follow-Up Period	Monthly Employment Rates (Figure 7.1)				Average Monthly Earnings (Figure 7.5)			
	Percentage Employed							
	Experimentals	Controls	Difference	p[a]	Experimentals ($)	Controls ($)	Difference	p[a]
Month 1	3.9	7.0	-3.1 ***	0.002	19	35	-16 ***	0.008
Month 2	5.7	8.6	-3.0 **	0.010	31	47	-16 **	0.034
Month 3	7.6	10.8	-3.2 **	0.014	41	53	-12	0.166
Month 4	8.9	12.1	-3.2 **	0.021	51	61	-10	0.255
Month 5	9.8	13.9	-4.0 ***	0.006	56	78	-22 **	0.031
Month 6	12.1	16.2	-4.1 ***	0.008	66	85	-19 *	0.090
Month 7	12.5	15.3	-2.8 *	0.073	71	95	-24 **	0.033
Month 8	13.2	15.6	-2.4	0.133	74	97	-23 *	0.054
Month 9	13.5	16.2	-2.8 *	0.088	77	99	-22 *	0.059
Month 10	13.6	15.6	-2.0	0.214	76	99	-23 **	0.045
Month 11	14.2	16.5	-2.3	0.164	81	99	-18	0.114
Month 12	14.1	16.7	-2.6	0.106	86	103	-17	0.163
Month 13	15.8	17.4	-1.6	0.351	94	108	-14	0.267
Month 14	16.5	19.2	-2.8	0.114	99	112	-13	0.314
Month 15	17.5	19.1	-1.6	0.382	109	124	-15	0.256
Month 16	19.4	20.2	-0.8	0.664	113	125	-12	0.389
Month 17	19.5	20.2	-0.7	0.701	112	133	-21	0.150
Month 18	19.0	19.3	-0.3	0.867	105	129	-24 *	0.073
Month 19	19.3	19.7	-0.4	0.849	110	129	-19	0.201
Month 20	17.8	18.1	-0.2	0.894	117	141	-24	0.130
Month 21	18.0	19.6	-1.5	0.394	127	156	-29 *	0.077
Month 22	18.5	17.9	0.6	0.742	134	142	-8	0.655
Month 23	18.9	18.4	0.5	0.774	138	143	-5	0.792
Month 24	19.5	19.1	0.4	0.836	149	153	-4	0.817
Month 25	20.7	20.7	0.0	0.993	158	166	-8	0.641
Month 26	21.8	21.7	0.1	0.956	167	178	-11	0.538
Month 27	23.4	23.0	0.4	0.819	181	189	-8	0.655
Month 28	24.4	25.8	-1.4	0.490	193	201	-8	0.682
Month 29	24.9	26.2	-1.3	0.523	202	210	-8	0.643
Month 30	25.9	26.7	-0.8	0.710	208	208	0	0.993
Month 31	26.0	27.5	-1.5	0.467	211	214	-3	0.875
Month 32	26.6	29.1	-2.5	0.220	220	230	-10	0.632
Month 33	29.4	30.4	-1.0	0.633	234	242	-8	0.703
Month 34	29.3	30.9	-1.6	0.454	242	247	-5	0.840
Month 35	29.8	30.7	-0.9	0.654	248	250	-2	0.933
Month 36	31.3	30.8	0.5	0.813	260	254	6	0.794
Month 37	32.1	31.2	0.9	0.677	264	259	5	0.796
Month 38	30.7	32.5	-1.7	0.419	260	261	-1	0.970
Month 39	30.5	33.5	-3.1	0.153	256	266	-10	0.654
Month 40	31.3	33.9	-2.6	0.235	261	272	-11	0.618
Month 41	32.2	34.3	-2.1	0.322	272	277	-5	0.831
Month 42	33.2	33.4	-0.2	0.928	283	274	9	0.697
Sample size	1,401	678			1,401	678		

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.6.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table G.8

Distribution of Job Duration in Weeks for New Chance Sample Members Within 42 Months After Random Assignment (Figure 7.2)

	Number of Jobs	
Weeks Ongoing	Frequency	Cumulative %
0	0	0.0
2	72	3.9
4	149	12.0
6	202	23.0
8	94	28.2
10	207	39.4
12	98	44.8
14	181	54.6
16	53	57.5
18	107	63.3
20	35	65.3
22	90	70.2
24	20	71.2
26	28	72.8
28	57	75.9
30	17	76.8
32	65	80.3
34	26	81.8
36	28	83.3
38	10	83.8
40	31	85.5
42	6	85.8
44	30	87.5
46	12	88.1
48	16	89.0
50	7	89.4
52	4	89.6
54	23	90.8
56	9	91.3
58	14	92.1
60	10	92.6
62	15	93.5
64	2	93.6
66	15	94.4
68	5	94.7
70	8	95.1
72	5	95.4
74	6	95.7
76	8	96.1
78	4	96.4
80	7	96.7
82	6	97.1

(continued)

Table G.8 (continued)

	Number of Jobs	
Weeks Ongoing	Frequency	Cumulative %
84	7	97.4
86	1	97.5
88	1	97.5
90	3	97.7
92	1	97.8
94	5	98.0
96	1	98.1
98	1	98.1
100	2	98.3
More	32	100.0
Sample size	1,836	

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for all 1,836 sample members for whom there were 42 months of follow-up survey data on job characteristics, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program. The reported sample sizes may fall short of this number because of missing or unusable items from some sample members' questionnaires.

Table G.9

Distribution of Average Hours Worked per Week for New Chance Sampl Members Within 42 Months After Random Assignment (Figure 7.3)

	Number of Jobs	
Average Hours Worked per Week	Frequency	Cumulative %
4	31	1.7
8	70	5.5
12	54	8.4
16	74	12.5
20	204	23.6
24	86	28.3
28	174	37.7
32	216	49.5
36	160	58.2
40	594	90.6
More	173	100.0
Sample size	1,836	

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.8.

Table G.10

Distribution of Average Hourly Wage for New Chance Sample Members Within 42 Months After Random Assignment (Figure 7.4)

	Number of Jobs	
Average Hourly Wage	Frequency	Cumulative %
$1	38	2.1
$2	58	5.2
$3	83	9.8
$4	186	19.9
$5	832	65.2
$6	328	83.1
$7	151	91.4
$8	72	95.3
$9	32	97.1
$10	26	98.5
$11	4	98.7
More	24	100.0
Sample size	1,834	

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.8.

Table G.11

Average Earnings of New Chance Sample Members Within Months 37-42 After Random Assignment, by Living Arrangement at 42 Months After Random Assignment (Figure 7.6)

Living Arrangement at Follow-Up	Total Earnings, Months 37–42 ($) Full Sample	Experimentals	Controls
Children only	1,255	1,263	1,293
Own mother, no partner	1,592	1,598	1,546
Husband	2,293	2,325	2,214
Partner	1,789	1,734	1,880
Other	1,605	1,573	1,583
Sample size	2,079	1,401	678

SOURCE: MDRC calculations from survey data.

NOTES: Calculations for this table used data for all 2,079 sample members for whom there were 42 months of follow-up survey data, including those with values of zero for outcomes and New Chance enrollees (i.e., experimentals) who did not participate in the program.

Table G.12

Average Earnings of New Chance Sample Members Within Months 37-42 After Random Assignment, by Living Arrangement and Fertility Status at 42 Months After Random Assignment (Figure 7.7)

Living Arrangement at Follow-Up	Sample Size	Total Earnings, Months 37-42 ($)
Children only		
Never pregnant	204	1,796
Pregnant, no birth	147	1,537
Gave birth	412	906
Own mother, no partner		
Never pregnant	93	2,177
Pregnant, no birth	65	1,886
Gave birth	181	1,146
Husband		
Never pregnant	52	2,955
Pregnant, no birth	51	3,200
Gave birth	162	1,843
Partner		
Never pregnant	97	3,289
Pregnant, no birth	97	1,532
Gave birth	240	1,282
Other		
Never pregnant	86	2,384
Pregnant, no birth	46	1,930
Gave birth	146	990

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.11.

Table G.13

Monthly Rates of AFDC Receipt for New Chance Sample Members Within 42 Months After Random Assignment (Figure 7.8)

	Percentage Receiving AFDC			
Follow-Up Period	Experimentals	Controls	Difference	p[a]
Month 1	95.2	92.6	2.6 ***	0.006
Month 2	94.8	91.7	3.1 ***	0.002
Month 3	93.9	90.8	3.1 ***	0.005
Month 4	93.2	89.2	4.0 ***	0.001
Month 5	91.6	88.1	3.5 ***	0.007
Month 6	90.7	88.2	2.6 *	0.052
Month 7	89.8	86.8	3.0 **	0.032
Month 8	89.9	86.9	3.0 **	0.031
Month 9	89.4	86.5	2.9 **	0.044
Month 10	88.6	86.8	1.8	0.217
Month 11	87.5	86.8	0.7	0.653
Month 12	87.0	86.4	0.6	0.710
Month 13	86.5	86.3	0.3	0.856
Month 14	86.1	85.9	0.2	0.902
Month 15	85.8	86.1	-0.3	0.847
Month 16	85.7	85.9	-0.2	0.893
Month 17	86.5	86.0	0.5	0.751
Month 18	86.1	85.7	0.4	0.797
Month 19	86.7	86.7	0.0	0.985
Month 20	84.6	84.9	-0.3	0.833
Month 21	83.9	84.1	-0.2	0.882
Month 22	84.1	83.2	0.9	0.585
Month 23	83.7	83.1	0.5	0.746
Month 24	83.3	82.0	1.3	0.417
Month 25	82.9	81.5	1.3	0.428
Month 26	82.0	81.1	0.9	0.580
Month 27	81.8	81.4	0.5	0.780
Month 28	80.9	80.7	0.2	0.925
Month 29	79.5	80.5	-1.0	0.591
Month 30	78.4	80.0	-1.6	0.365
Month 31	78.2	79.6	-1.5	0.417
Month 32	77.4	77.9	-0.5	0.772
Month 33	77.6	77.2	0.4	0.842
Month 34	76.9	76.9	0.0	0.979
Month 35	76.9	76.7	0.2	0.926
Month 36	75.9	76.2	-0.3	0.855
Month 37	75.4	74.7	0.7	0.693
Month 38	75.5	73.7	1.9	0.322
Month 39	75.6	74.3	1.3	0.489
Month 40	75.8	74.7	1.1	0.556
Month 41	76.0	74.4	1.6	0.389
Month 42	75.4	73.5	2.0	0.301
Sample size	1,401	678		

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.6.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

Table G.14

AFDC Receipt by New Chance Sample Members in Month 41 After Random Assignment, by Living Arrangement and Employment Status at 42 Months After Random Assignment (Figure 7.9)

	Percentage Receiving AFDC	
Living Arrangement at Follow-Up	Among Those Employed	Among Those Not Employed
Children only	54.2	91.6
Own mother, no partner	42.3	80.7
Husband	27.0	45.7
Partner	38.7	81.1
Other	38.2	76.3
Sample size	2,079	

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.11.

Table G.15

Use of Market Child Care by New Chance Sample Members Within 42 Months After Random Assignment (Figure 8.1)

	Percentage Using Market Child Care			
Follow-Up Period	Experimentals	Controls	Difference	p^a
Month 1	42.5	9.7	32.8 ***	0.000
Month 2	53.2	13.1	40.1 ***	0.000
Month 3	53.1	16.7	36.3 ***	0.000
Month 4	50.4	19.1	31.3 ***	0.000
Month 5	46.6	20.7	25.9 ***	0.000
Month 6	43.3	21.8	21.5 ***	0.000
Month 7	40.0	21.1	18.8 ***	0.000
Month 8	38.0	21.2	16.8 ***	0.000
Month 9	36.1	22.3	13.8 ***	0.000
Month 10	33.2	22.1	11.0 ***	0.000
Month 11	31.8	21.4	10.4 ***	0.000
Month 12	29.7	20.5	9.2 ***	0.000
Month 13	29.6	20.5	9.1 ***	0.000
Month 14	27.4	20.3	7.1 ***	0.001
Month 15	27.2	19.8	7.4 ***	0.001
Month 16	26.7	19.9	6.8 ***	0.001
Month 17	26.8	22.0	4.8 **	0.027
Month 18	26.2	21.4	4.8 **	0.027
Sample size	1,401	678		

SOURCE: MDRC calculations from survey data.

NOTES: See Table G.6.

Market child care includes care at a day care center, a preschool, a family day care home, or by a paid babysitter.

[a] A two-tailed t-test was applied to each regression-adjusted difference between average experimental and control group outcomes. The column labeled "p" is the statistical significance level of the difference between experimental and control group outcomes: That is, p is the probability that average outcomes are different only because of random error. Statistical significance levels are indicated as *** = 1 percent; ** = 5 percent; * = 10 percent.

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Selected Publications on MDRC Projects

Programs for Teenage Parents

The New Chance Demonstration

A test of a comprehensive program of services that seeks to improve the economic status and general well-being of a group of highly disadvantaged young women and their children.

New Chance: Implementing a Comprehensive Program for Disadvantaged Young Mothers and Their Children. 1991. Janet Quint, Barbara Fink, Sharon Rowser.

Lives of Promise, Lives of Pain: Young Mothers After New Chance. Monograph. 1994. Janet Quint, Judith Musick, with Joyce Ladner.

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Parenting Behavior in a Sample of Young Single Mothers in Poverty: Results of the New Chance Observational Study. 1997. Martha Zaslow, Carolyn Eldred, editors.

The LEAP Evaluation

An evaluation of Ohio's Learning, Earning, and Parenting (LEAP) Program, which uses financial incentives to encourage teenage parents on welfare to stay in or return to school.

LEAP: Implementing a Welfare Initiative to Improve School Attendance Among Teenage Parents. 1991. Dan Bloom, Hilary Kopp, David Long, Denise Polit.

LEAP: Interim Findings on a Welfare Initiative to Improve School Attendance Among Teenage Parents. 1993. Dan Bloom, Veronica Fellerath, David Long, Robert Wood.

LEAP: The Educational Effects of LEAP and Enhanced Services in Cleveland. 1994. David Long, Robert Wood, Hilary Kopp.

LEAP: Three-Year Impacts of Ohio's Welfare Initiative to Improve School Attendance Among Teenage Parents. 1996. David Long, Judith Gueron, Robert Wood, Rebecca Fisher, Veronica Fellerath.

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Project Redirection

A test of a comprehensive program of services for pregnant and parenting teenagers.

The Challenge of Serving Teenage Mothers: Lessons from Project Redirection. Monograph. 1988. Denise Polit, Janet Quint, James Riccio.

The Community Service Projects

A test of a New York State teenage pregnancy prevention and services initiative.

The Community Service Projects: Final Report on a New York State Adolescent Pregnancy Prevention and Services Program. 1988. Cynthia Guy, Lawrence Bailis, David Palasits, Kay Sherwood.

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Reforming Welfare

Books and Monographs

Reforming Welfare with Work (Ford Foundation). Monograph. 1987. Judith Gueron. A review of welfare-to-work initiatives in five states.

From Welfare to Work (Russell Sage Foundation). Book. 1991. Judith Gueron, Edward Pauly. A synthesis of research findings on the effectiveness of welfare-to-work programs. Chapter 1, which is the summary of the book, is also published separately by MDRC.

Five Years After: The Long-Term Effects of Welfare-to-Work Programs (Russell Sage Foundation). Book. 1995. Daniel Friedlander, Gary Burtless. An analysis of five-year follow-up data on four welfare-to-work programs.

After AFDC: Welfare-to-Work Choices and Challenges for States. Book. 1997. Dan Bloom. A summary and synthesis of lessons derived from studies of welfare reform programs.

ReWORKing Welfare: Technical Assistance for States and Localities

After AFDC: Welfare-to-Work Choices and Challenges for States. See under Books and Monographs.

Changing to a Work First Strategy: Lessons from Los Angeles County's GAIN Program for Welfare Recipients. 1997. Evan Weissman.

Work First: How to Implement an Employment-Focused Approach to Welfare Reform. 1997. Amy Brown.

Connections to Work Project

Alternative approaches to help welfare recipients and other low-income populations access and secure jobs.

Tulsa's IndEx Program: A Business-Led Initiative for Welfare Reform and Economic Development. 1997. Maria Buck.

Working Papers

Working Papers related to a specific project are listed under that project.

Learning from the Voices of Mothers: Single Mothers' Perceptions of the Trade-offs Between Welfare and Work. 1993. LaDonna Pavetti.

Unpaid Work Experience for Welfare Recipients: Findings and Lessons from MDRC Research. 1993. Thomas Brock, David Butler, David Long.

From Welfare to Work Among Lone Parents in Britain: Lessons for America. 1996. James Riccio.

Papers for Practitioners

Assessing JOBS Participants: Issues and Trade-offs. 1992. Patricia Auspos, Kay Sherwood.

Linking Welfare and Education: A Study of New Programs in Five States. 1992. Edward Pauly, David Long, Karin Martinson.

Improving the Productivity of JOBS Programs. 1993. Eugene Bardach.

Reports and Other Publications

Time-Limited Welfare

Florida's Family Transition Program

A study of Florida's time-limited welfare program.

The Family Transition Program: An Early Implementation Report on Florida's Time-Limited Welfare Initiative. 1995. Dan Bloom.

The Family Transition Program: Implementation and Early Impacts of Florida's Initial Time-Limited Welfare Program. 1997. Dan Bloom, James Kemple, Robin Rogers-Dillon.

The Cross-State Study of Time-Limited Welfare

An examination of the implementation of some of the first state-initiated time-limited welfare programs.

Implementing Time-Limited Welfare: Early Experiences in Three States. 1995. Dan Bloom, David Butler.
The View from the Field: As Time Limits Approach, Welfare Recipients and Staff Talk About Their Attitudes and Expectations. 1997. Amy Brown, Dan Bloom. David Butler.

Making Work Pay

The Minnesota Family Investment Program (MFIP)

An evaluation of Minnesota's welfare reform initiative.

MFIP: An Early Report on Minnesota's Approach to Welfare Reform. 1995. Virginia Knox, Amy Brown, Winston Lin.
Making Welfare Work and Work Pay: Implementation and 18-Month Impacts of the Minnesota Family Investment Program. 1997. Cynthia Miller, Virginia Knox, Patricia Auspos, Jo Anna Hunter-Manns, Alan Orenstein.

The New Hope Project

A test of a neighborhood-based antipoverty program and welfare alternative operating in Milwaukee.

The New Hope Offer: Participants in the New Hope Demonstration Discuss Work, Family, and Self-Sufficiency. Working Paper. 1996. Dudley Benoit.
Who Got New Hope? Working Paper. 1997. Michael Wiseman.
Creating New Hope: Implementation of a Program to Reduce Poverty and Reform Welfare. Thomas Brock, Fred Doolittle, Veronica Fellerath, Michael Wiseman.

Canada's Self-Sufficiency Project (SSP)

A test of the effectiveness of a temporary earnings supplement on the employment and welfare receipt of public assistance recipients. Reports on the Self-Sufficiency Project are available from: Social Research and Demonstration Corporation (SRDC), 275 Slater St., Suite 900, Ottawa, Ontario K1P 5H9, Canada. Tel.: 613-237-4311; Fax: 613-237-5045. In the United States, the reports are also available from MDRC.

Making Work Pay Better Than Welfare: An Early Look at the Self-Sufficiency Project (Social Research and Demonstration Corporation). 1994. Susanna Lui-Gurr, Sheila Currie Vernon, Tod Mijanovich.
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JOBS Programs

The National Evaluation of Welfare-to-Work Strategies

An evaluation of welfare-to-work programs launched under the Job Opportunities and Basic Skills Training (JOBS) provisions of the Family Support Act of 1988.

From Welfare to Work (Russell Sage Foundation). See under Books and Monographs.

The Saturation Work Initiative Model in San Diego: A Five-Year Follow-up Study. 1993. Daniel Friedlander, Gayle Hamilton.

The JOBS Evaluation: Early Lessons from Seven Sites (U.S. Department of Health and Human Services [HHS]). 1994. Gayle Hamilton, Thomas Brock.

Five Years After: The Long-Term Effects of Welfare-to-Work Programs. See under Books and Monographs.

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